

Contemporary Psychology

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Dreams Reflect the Solvings of Problems

Thomas M. French

The Integration of Behavior. Vol. III: *The Reintegrative Process in a Psychoanalytic Treatment*. Chicago: University of Chicago Press, 1958. Pp. xii + 484. \$10.00.

Reviewed by ERNEST R. HILGARD

Dr. Hilgard, who has been a professor of psychology at Stanford University for twenty-five years, is best known for his work on learning (with D. G. Marquis, *Conditioning and Learning*, Appleton-Century-Crofts, 1940; by himself, *Theories of Learning*, same publisher, 2 ed., 1956), but he has also long been concerned with the psychology of motivation and thus with psychoanalysis, as witness the relevant chapters in the two books just cited. In 1952 he contributed a chapter to the Hixon Symposium, *Psychoanalysis as Science* (Stanford Univ. Press). He was President of the American Psychological Association in 1949. In ten years he will be one of American psychology's elder statesmen.

THE appearance of the third volume of this five-volume work permits an assessment midstream of a bold and painstaking attempt to deal with the step-by-step and cyclical processes that go on in the patient, and between the patient and the therapist, in the midst of psychoanalytic treatment. Dr. French is particularly well qualified to make his interpretations understandable to both

psychoanalysts and general psychologists because of his long interest in bridging their disciplines. He has been one of the leading figures in the Chicago Institute for Psychoanalysis since 1932, following return from his training in the Berlin Psychoanalytic Institute. Psychologists will be interested in his frequent acknowledgments of the work and ideas of Pavlov, Köhler, Tolman, and Kurt Lewin.

(Dr. French's first two volumes, which are also under consideration here, are: I, *Basic Postulates*, 1952, pp. xi + 272, and II, *The Integrative Process in Dreams*, 1954, pp. xi + 367, both also by the University of Chicago Press.)

The data pool upon which Dr. French draws is the analysis of a single patient who was in treatment with Dr. Helen V. McLean for 303 hours between 5 December 1935 and 15 July 1938. Dr. McLean provided her notes for Dr. French's retrospective analysis. Because these notes give her 'on-the-spot' interpretations as well as the patient's remarks, we have the advantage of both her suggestions and those that Dr. French arrives at upon reflection.

Primary attention is given to the care-

fully reported dreams. The over-all plan is to approach the material somewhat differently in each volume. In the later volumes the material previously presented is reviewed from a fresh standpoint, and new material is then discussed according to 'predictions' made on the basis of what has gone before. Dr. French knows when he is 'predicting' that he is already aware of the outcome, but he shows how one might go about it with a new case for which the later developments are unknown. In these three volumes we have not yet gone beyond the 207th hour of the 303 total hours, and most of the interpretations have been based on the 167 hours of the first two years. The two volumes that are still to come will deal with a 'factor analysis' (not the familiar statistical kind, however), and with the biological foundations. (The books follow each other somewhat as the installments of a continued story in a magazine, with a synopsis in the later volumes sufficient for one to read the third volume, for example, without having the others at hand.)

THE recurrent theme is that a patient, in the analyst's presence, and presumably with his help, is trying to become a problem-solving person. His dreams reflect his grasp of the conflict situation and show his contemporary efforts at conflict resolution.

... the dream work, like the thought processes directing our ordinary waking activity, is dominated by the need to find a solution for a problem. (Vol. II, p. 72.)

All problems that reach solution do so through a combination of motivation

and insight—motivation providing the pressure to move toward a goal, and insight activating complexly organized efforts to solve a problem or to put a plan into execution. Dr. French believes that psychoanalysis in the past has been chiefly interested in the motivation of dreams (i.e., their wish fulfillment), whereas he tries to show that they also have a logical structure, and that the logical structures of a series of dreams of the same person are interrelated. Thus dreams share the same processes that we find in ordinary waking behavior and in neurotic behavior.

The cognitions with which classical psychoanalysis is concerned are chiefly insights into unconscious processes and mechanisms. Dr. French believes that a more differentiated meaning of cognition is needed. The awareness of a previously repressed thought or wish is a specialized and restricted kind of insight. It can be called "introspective insight" and it differs from the "practical understanding" that is the insight governing the actual behavior and also from the "problem-solving insight" that is what the patient needs to know in order to achieve his goals or to solve his problems. Furthermore, the insight that exerts a therapeutic effect need not be conscious.

'Unconscious insight' is 'practical understanding' that does not happen to be accompanied by conscious awareness. (Vol. 3, p. 20.)

The integrative processes that give the title to the series of volumes are related, of course, to psychoanalytic ego psychology, but Dr. French believes that his methods are more empirical than the classical ones using the id, ego, and superego structure. He is not content to give the standard name *ego* to the integrative function because he wishes to call attention to its complexity, to its component parts. He feels that his interpretations arise directly from the study of integrative functions as he sees them actually at work, rather than as a result of reasoning from theory. The method he uses is said to be very matter of fact, a trying to reduce all the circumstances of a patient's situation to a point of such clarity that we can intuit why he does what he



THOMAS M. FRENCH

does: given anyone else caught in his circumstances, with his history, they would do just what he is doing. If we have reached this level of understanding, then, and only then, have we reached a satisfactory interpretation of his behavior. This goal is more easily stated than achieved, and when, as in these volumes, we try to understand what is happening through the course of events in a series of dreams, we are never quite sure when we are dealing with simple observables and when we are dealing with inferences.

An early dream (one brought into the tenth hour of analysis) will make the method more concrete. The patient (now a man of 46) dreams of his school days. He was walking home and crossing a bridge. There he saw a girl leaning on the bridge, watching the boats. He pinched her playfully, then walked her home to her mother, who rewarded him with a glass of milk. The dream is said to be an attempt to find a solution to the problem that is troubling the man in his contemporary life. The interruption of the sexual advance to the girl (the pinching) represents his inhibited sex life, which is conditioned by his need to placate (and to depend upon) a mother figure. In the dream the mother rewards him (with milk!) for protecting her daughter and bringing her home safely instead of attacking her sexually. Just as in this case, many dreams mirror quite faithfully the progress of therapy, the conflict uppermost at the time, the nature of the transference to the analyst, gains in self-confidence, reality testing, and so on.

Yet, even as in the interpretations above, there are always steps of inference. Here is where the acceptance of common-sense and trusted intuition comes into play. The pinch is alternatively interpreted elsewhere as a hostile gesture; this is all right, in view of the well-known principles of condensation and displacement. But this is scarcely raw empiricism.

DR. FRENCH is clearly a Freudian. This fact is evident enough in his emphasis upon early sexual attachments to the parents, on the aftereffects of having witnessed the primal scene, on bisexuality, on dream symbolism, on transference as a strong feature of therapy, on symbolic representation of conflict through bodily symptoms. But his differences from classical psychoanalysis show him to be an independent thinker, one who does not simply recite the catechism of the faithful. He rejects Freud's formulation of primary process thinking in dreams, in which energy is displaced along any available pathway without regard to logic, in favor of interpreting the dream as an organized process. He believes that the usual Freudian conception of regression is misleading because it implies a misconception of the relation between the present and the past. The past is used in rational problem solving as well as in neurotic behavior; regression is not a return to the past but a failure to subordinate patterns from the past. Also regression is commonly used in an evaluative sense, to imply something primitive, infantile, less good than some more mature way of behaving, whereas in the progress of psychotherapy the patient has to go through a number of cyclical changes in order to achieve integrative solutions to his problems, and his regressive phases may be as good for his purposes as any other. Finally, Dr. French's interpretation of transference, while coherent with Freud's teachings, is not a strictly regressive-transference theory, such as is described in Karl Menninger's *Theory of Psychoanalytic Technique* (1958). Transference remobilizes an earlier traumatic conflict under conditions that make possible a resumption of the learning process. For the patient to be helped, he must undergo a corrective emotional experience

based on confronting his infantile wishes with present reality.

Dr. French rightly believes that psychoanalysis can substantiate its claims as a science only if it reviews its data from several points of view before becoming committed to a fixed mode of organizing the data. The theoretical structure of any empirical science is always a provisional one, guiding the search for new facts, itself being modified by these facts. Dr. French is here contributing to the evolution of a science of psychoanalysis.

There are at the present time a number of investigations going on with the aid of electromagnetic tape and motion pictures to provide a detailed record of what happens in psychoanalytic interviews. These efforts will provide better detailed data than the notes from which Dr. French had to work. The volume of interpretation needed

to clarify what was happening in the sessions of Dr. French's subject gives pause, however, to those who hope for a microscopic, moment-by-moment analysis of movement in psychoanalytic treatment. What we learn from his model is that some simplifying assumptions will have to be made if a communicable picture of the process is to be made. It may be necessary to sacrifice some richness of detail if the basic processes are to be visualized at all.

Dr. French's pioneer effort provides an important model. One cannot dispose of it glibly because it is 'only one case,' for it is essential to the understanding of what goes on that the dreams and experiences be those of a single person. Few investigators will have Dr. French's patience, but other cases studied as his has been by those who have the patience will contribute greatly to our understanding not only of psychoanalysis but of human problem solving in general.

point was selected. In this instance, financial conditions unfortunately prevailed over scientific considerations. A North Carolina county, significantly different from most Southern counties, was chosen. From this atypically wealthy, educated, and urbanized region a random sample of the male working force was obtained as interviewees. Northern students, invading the sanctity of Southern households, posed a number of questions concerning various aspects of the respondent's attitudes towards Negroes and desegregation. In similar fashion, they questioned the interviewees regarding a number of socioeconomic dimensions—occupation, education, religion, residence, age, exposure to mass media, etc.

In a preliminary analysis, all socioeconomic variables were indiscriminantly used as predictors of differences in attitude. On the basis of this empirical analysis, the three 'best' predictors—education, occupation, and exposure to mass media—were selected as the major independent variables for further analysis. In similar fashion five 'lesser' predictors were included as control variables, and the remainder jettisoned as insignificant.

More than half of the book is devoted to a somewhat tedious review of the relative importance of each of the independent variables in producing significant differences in attitude. Table after table presents mean scores and *t* tests for categories within a major predictor while controlling for each of the other independent and control variables. The appropriateness of *t* tests or other parametric statistics is unfortunately never established. It would have been better to have employed an analysis of variance, so that the significance of interaction could have been specifically determined.

THERE is one ingenious theoretical and methodological part of the study which rescues it from being merely another in a long list of atheoretical, descriptive, exploratory surveys. Tumin has formulated and proceeded to measure five dimensions of attitude: (a) *The Image*—the white's mental picture of the characteristics of Negroes, (b) *The Ide-*

Who Can Accept Change?

Melvin M. Tumin

Desegregation: Resistance and Readiness. Princeton, N. J.: Princeton University Press, 1958. Pp. xviii + 270. \$5.00.

Reviewed by CHARLES G. McCLINTOCK

Dr. McClintock, an assistant professor of psychology at the University of California, Santa Barbara, obtained his PhD a few years ago in the University of Michigan's Social Psychology Program where he worked on the formation and change of attitudes.

ON May 17, 1954, the United States Supreme Court ruled unanimously that public schools should be desegregated "with all deliberate speed." To the social scientist, the decision was noteworthy. By taking into consideration the testimony and research findings of psychologists and sociologists, the Court's decision escaped the narrow confines of traditional legalism. It maintained that segregated schools could not be 'equal' because of the adverse social and psychological effects of separateness upon both Negro and white. In addition, it heralded the arrival of a

period of sharp social conflict in the South. In effect the social scientist was given clear notification of a dramatic opportunity to develop and test theories of individual and group behavior under conditions of conflict and change.

One social scientist who responded to this challenge was Melvin Tumin, a sociologist who has spent many years studying the relationships between minority and majority groups. Recognizing a significant opportunity for research, and faced with the necessity of training students in field methodology, Tumin directed a systematic study of attitudes toward Negroes and desegregation in one Southern county. This volume represents an elaborate description of the theory, methods, and findings of this study.

The over-all design of the project falls within the familiar pattern of traditional survey methodology. A sampling

ology—what types of social relations the white would prefer with Negroes, (c) *Sentiment Structure*—how would the white feel if the social relation occurred, (d) *General Action Set*—what would the white do if the relation occurred, and (e) *Specific Action Set*—what would the white do to facilitate or resist desegregation of schools. These dimensions can be described in terms of a continuum with private-prejudicial opinions, on one end, and public-discrimination, on the other. The continuum seems to represent a marriage between the early public-private dichotomy of attitudes formulated by Schanck and Katz, and the recent work in the area of minority groups which differentiates between prejudice and discrimination.

Utilizing these five attitude dimensions as dependent variables, the research yields a number of interesting findings. (1) One cannot with high reliability predict from the prejudice end of the continuum (*Image*) to the discrimination end (*Specific Action*). (2) All subgroups falling within the three major predictors as well as the total sample showed lower segregationist scores on the *Specific Action* than on the *Image* end of the continuum. (3) Attitudinal differences between groups were greater for *Specific Action* than for *Image*. Finally (4) the higher the educational level, the more exposure to mass media, and the higher the occupational level, then the less over-all discrimination found, the more quickly did segregation scores decline on the continuum from *Image* to *Action*, and the greater was the disparity between scores on *Image* and *Action*. Tumin concludes that those who, because of education and exposure to mass media, are more aware of the negative social consequences of resistance to desegregation, as well as those whose status is less jeopardized by desegregation, are the most ready for desegregation.

Towards the end of the book, the author takes one chapter to discuss a separate panel study in which 28 community leaders were interviewed. The findings indicate that leaders in this progressive Southern community are about evenly divided in terms of those who favor and those who oppose desegre-

gation. More importantly, the findings show that if legitimate leaders are active and outspoken, they can enforce moral limits upon public debate and action, and prevent the violence and antisocial behaviors characteristic of a Little Rock. The author seems unrealistically optimistic, however, in maintaining that what goes on in this wealthy, highly educated, and atypically liberal Southern community can be generalized to other Southern communities.

Upon completing the book, this reviewer had the distinct impression that the significance of the study had been

inflated. Although ingenious in parts, the research certainly does not seem to justify two hundred and seventy pages of discourse. Judicious editing could have reduced it enough to save the reader time and perhaps to hold his interest. Nor does the author's stated intention of writing for both layman and scientist succeed, for both will find much of the theory confusing, and most of the analysis labored. We could do with much better accounts of what will surely come to be seen as one of the most dramatic periods of social change in the United States.

Hydrodynamic Psychology

A. V. Wolf

Thirst: Physiology of the Urge to Drink and Problems of Water Lack.
Springfield, Ill.: Charles C Thomas, 1958. Pp. x + 536. \$12.50.

Reviewed by PAUL S. SIEGEL

Dr. Siegel is Professor of Psychology and Chairman of the Department at the University of Alabama. He has a PhD from the University of North Carolina, has been interested in learning and motivation all along, publishing articles in that field, including a half dozen on hunger and thirst, and he has recently completed for the U. S. Quartermaster Corps a study of The Monotony Effect in Food Acceptance (when adaptation to a boarding house is negative).

THE author's credentials are of the finest. He is eminently qualified to write this book: as present Chief of the Renal Section of the Walter Reed Army Institute of Research, as a then-member of E. F. Adolph's World War II team of researchers on thirst and heat exhaustion in the southwestern deserts of the U.S.A., as a twenty-year student of the subject.

He states his purpose as follows: "Although its roots lie deep in the most active issues of water metabolism, diposology itself has been a neglected province of physiology and pathology. The purpose of this book (and its companion volume by J. H. Holmes, *Thirst:*

Clinical Pathology and Medical Aspects) is to improve this situation by providing a store of physiologic and medical knowledge of thirst. Its pages necessarily treat of theory, experiment, observation, and lore, and of suffering, survival, and death. They consider practical problems of obtaining potable fluid from sea ice, fish, and cacti not less than the perplexing circumstances of Buridan's ass. A few unorthodox matters, such as 'volume regulation,' have also seemed properly to merit inclusion in a comprehensive syntax. In all, I have tried to place a great many subjects in such order and deal with them in such ways as has seemed worthwhile."

This purpose is realized—indeed with a vengeance, for this is an encyclopedic treatment. One wonders what is left for J. H. Holmes. The psychologist will lament the short shrift given Buridan's ass. And most will object to the inclusion of Part II, some 90 pages devoted almost totally to accounts—some classic, some little known—of thirst in the human at the extremes of death and near-death. These reports are not without reader excitement and they are done vividly enough to leave one with the

firm resolve not to get caught in a mess like that; but, few will share the author's conviction that this material, "used carefully, can substantially forward scientific dissection of the problems of thirst."

The author treats of matters below, above, and on both sides of his basic issue, but not to its neglect. He has done a handsome organizational job: no mean accomplishment in view of the antiquity of the problem, the diversities of definition and of level of discourse, the species differences, and the entanglement of the main issue with cognate ones. The virtues of single as opposed to multiple-authored treatment is in immediate evidence when one compares this book with the recent monograph on hunger (*Annals New York Acad. Sci.*, 1955, 63, art. 1). With an impressive scholarship, Wolf has wrung the subject of thirst quite dry. If a treatment of this scope appears again within the next 25 years, it will be at the cost of an unforgivable redundancy.

A style, best described as near classic, takes form within this technical treatise. If Wolf knows the message of Rudolph Fleisch, he entertains no enthusiasm for such carrying-on. He presumes a measure of reader literacy. And some will even pause to savor the more than occasional delicious phrase typified in the following description of osmosis: "To the physical chemist, it is merely the consequence of the tendency of two different liquid species, under the impulse of thermal agitation, to achieve a state of maximum disorder by any available path." Damn the Fleisch Count. Full speed ahead!

Wolf finds it possible to write more than 135 pages without recourse to formal definition. He relies upon context to establish meaning. The quotations that follow are instructive: "Clearly, a manifest urge to take water, which is sometimes referred to as thirst, may reflect something other than a sensation or percept" (p. v). "There may be present simply, but definitely, an urge to drink, hard to describe but not vague, perhaps not unlike an urge to eat a sour pickle" (p. 96). When he comes to grips with the problem (pp. 140-143), the outcome is disappointing, albeit predictable. Definition he handles too poorly

for his treatment to withstand critical assessment. The extent to which this indifference to formality weakens his presentation may be pondered. It is entirely clear that, at ultimate, Wolf is dealing with two imperfectly correlated dependent variables: the verbal testimony of the human subject and drinking behavior human and infrahuman. In general, the urge for formal rigor seems to be inversely related to the extent of knowledge about external control.

THE author is persuaded to accept a theory of osmometric or cellular dehydration, one which readily encompasses recent findings on the profound alteration of drinking behavior that attends the stimulation or ablation of regions within the hypothalamus. Thus he regards this statement as paramount: "A high correlation between the intensity of thirst and the degree of hypertonia or increased effective osmotic pressure of body fluids is significant beyond cavil. By extension, the correlation between thirst and the diminished water content of general body cells and/or osmoreceptors is equally remarkable" (p. 103). His own principal contribution to the osmometric hypothesis (developed in earlier journal articles) is found in an ingenious predictive equation which lends itself to quantitative extension (p. 75). Using it, he is able to determine a thirst threshold in man and dog following the infusion of saline solutions.

Wolf acknowledges that the osmoreceptor theory is a limited one. It does not handle well the thirst that attends salt deficiency, self-regulation (men and rats may prefer dilute saline solutions), nor the facts of satiety ("diverse mechanisms appear to underlie satiety and thirst," p. 149). His incorporation of the dry-mouth theory is intriguing, the more so in view of the current and almost desperate inability of the psychologist to demonstrate the acquisition of a learned drive of the appetitive sort. He speculates that drinking or the thirst sensation evoked by buccal dryness may be a "thirst reflex" (p. 95), that arises out of an historical association with stimulation of the osmoreceptors (the unconditioned or biologically adequate state of affairs).

All but the physiologically most sophisticated will find the going rough in spots, but even the least sophisticated will profit from a careful reading. Fortunately, much of the technical discourse is presented in parallel or else is unessential to the main development. Cross-references to earlier and later related discussion are helpful and liberally employed. The glossary is a must for there are some uncommon terms found in the science of dipsology, e.g., *aptyalia*, *xerostomia*. The substance of this book is contained in Chapters I-III (*Some Fundamental Aspects of Body Fluid, Thirst and Drinking, Satiety*). Chapter IV (*The Regulation of Fluid Volume*) is definitely 'off-beat' (Wolf also poses a reservation here). I should vote for its inclusion.

Chapter V (*Problems of Thirst*) invites particular comment, for it is concerned principally with the practical aspects of survival under conditions of short water supply. The traveler, anticipating jeopardy of this sort, may take comfort if he learns the rules. The chapter treats of such matters as the drinking of sea water and admixtures of fresh and sea water, of potifying sea water, of absorption through the skin. Drinking and water balance in marine and desert mammals, and in birds and fish, are also treated in this chapter. This reviewer gets at least a weak feeling that the chapter is tacked on, but again I vote for its inclusion.

THE book will find its way into all main libraries and into the personal libraries of many physicians, physiologists, and psychologists. On the academic scene, it will be utilized often as a reference source, seldom as a text. For the narrow-but-deep seminar, it will serve admirably with its asset of comprehensive coverage under one jacket.

In 1947, in their *Physiology of Man in the Desert*, E. F. Adolph et al. observed: "We have tried to make use of the current theories about the urge of thirst. . . . It can be concluded that in practice the theories regarding thirst have proven useless" (p. 253). If recast today, this statement would necessarily be weaker. Wolf's scholarship leaves the situation quite a bit tidier.

Paperbound Books in the Classroom, III

By CALVIN S. HALL

Dr. Hall, now of Syracuse University but about to take leave for a spell of writing, has already twice covered psychology's paperbound books for CP, first in CP, Oct. 1956, 1, 291-294, and second in CP, Nov. 1957, 2, 275-278. The present report, which takes us up to the spring of 1959, will be his last, and, if CP's readers wish CP to continue to cover the paperbounds, they had better write in to say so. Hall is also CP's dream specialist, for he has reviewed Bertram Lewin on dreams (CP, June 1959, 4, 164f.), and his own book, *The Meaning of Dreams*, should have been published by Dell in paper-cover before this review appears. He also has a general psychology coming along next fall with Howard Allen—a quarter of a million words, he says.

THE principal trend in the publication of paperbound psychology books since my last review (1957) is the larger proportion of relatively high-priced, quality paperbounds being published. In the first review of paperbounds (1956) 85 per cent of the books were priced at less than one dollar; in the second, 58 per cent were under a dollar. Of the 59 books listed below only 36 per cent retail for less than a dollar. Prices are going up. It is a regrettable trend because the chief virtue of the paperbound has been its low price. A teacher might ask students to buy ten fifty-cent volumes, but he is not going to expect them to buy ten volumes which retail at an average price of \$1.50. Fortunately, Mentor and Penguin are still publishing low-priced paperbounds of good quality.

Books in the area of abnormal psychology, psychology of personality, psychoanalysis, and psychotherapy predominate. Books by Freud, twelve of them, lead the field by a wide margin. General and theoretical psychology are slowly becoming represented, however. We welcome, in particular, James' *Principles*,

Tolman's *Collected Papers* entitled *Behavior and Psychological Man*, Watson's *Behaviorism*, and Allport's *The Nature of Prejudice*.

R. R. Bowker got out a college edition of their *Index of Paperbound Books* in the spring of 1958, and distributed it widely to college teachers. Let us hope they do it again. (They have.)

The *New York Herald Tribune* published a special paperback section January 18, 1959, which contained many interesting articles. Authorities in various fields were asked to prepare lists of books that could be purchased for approximately ten dollars. John Dollard prepared the list for psychology. It is reproduced here.

<i>Animals and Men</i> , by D. KATZ (Penguin)	\$.50
<i>Basic Teachings of the Great Psychologists</i> , by S. STANSFELD SARGENT (Barnes and Noble)	1.50
<i>Child Care and the Growth of Love</i> , by JOHN BOWLBY (Pelican)	.85
<i>An Elementary Textbook of Psychoanalysis</i> , by CHARLES BRENNER (Anchor)	.95
<i>The Fifty Minute Hour</i> , by ROBERT LINDNER (Bantam)	.35
<i>General Introduction to Psychoanalysis</i> , by SIGMUND FREUD (Pocket Books)	.50
<i>Hamlet and Oedipus</i> , by ERNEST JONES (Anchor)	.85
<i>William James</i> , by MARGARET KNIGHT (Penguin)	.65
<i>Language and Thought of the Child</i> , by JEAN PIAGET (Meridian)	1.35
<i>The Origins of Psychoanalysis</i> , by SIGMUND FREUD (Anchor)	1.25
<i>Psychology of Sex</i> , by HAVELOCK ELLIS (New American Library)	.50
<i>Uses and Abuses of Psychology</i> , by H. J. EYSENCK (Penguin)	.85
Twelve books for	\$10.10

The books listed below are grouped under several subject headings, and price ranges are indicated by stars. Three stars (***) signifies a price range of fifty cents or less, two stars (**) a price range from over fifty cents up to and including a dollar, and one star (*) all prices over a dollar. The addresses of the publishers of the paperbound books reviewed here are listed at the end of this article. Paperbound editions can be ordered from the publisher by referring to the name of the series and the number which appears between parentheses in most of the citations. Publishers will usually send examination copies upon request.

Child and Educational Psychology

** BOWLBY, J. *Child Care and the Growth of Love*. Penguin. \$.85.

Not seen.

* MONTAGU, A. *Education and Human Relations*. (Evergreen, E-87.) Grove, 1958. Pp. 191. \$1.45. Reprinted and original articles.

Montagu is a prolific writer. In this volume, he discourses on democracy, education, human nature, women's education and lots else. No index.

* PIAGET, J. *The Language and Thought of the Child*. (M 10.) Meridian, 1955. Pp. 251. \$1.35. Unabridged. Reprint.

It is good to have this 'classic' available in an inexpensive edition. No index.

*** VANDIVERT, W., VANDIVERT, RITA, AND BURGER, C. *Common Wild Animals and Their Young*. (LC105.) Dell, 1957. Pp. 128. \$.50. Original.

This delightful book contains 12 wonderful photographs and 30 line-drawings of the young of various species and their parents. The accompanying text belongs to the natural history genre. It would be an innovation to use this book in courses in child psychology. No index.

Counseling and Psychotherapy

* BEUKENKAMP, C., JR. *Fortunate Strangers*. (Evergreen, E-137.) Grove, 1958. Pp. 269. \$1.95. Unabridged. Reprint.

This beautifully titled book is a popularly written account of eight people in group psychotherapy. It reads like a novel. No index.

* BYCHOWSKI, G. AND DESPERT, J. LOUISE. (Eds.) *Specialized Techniques in Psychotherapy*. (Evergreen E-91.) Grove, 1958. Pp. 371. \$1.95. Reprint. Unabridged.

Chapters on narcodiagnosis and narcotherapy (Hoch and Polatin), hypnoanalysis (Lindner), telepathy (Eisenbud), art therapy (Stern), group therapy (Spotnitz), marital problems (Mittelman), child therapy (Beata Rank, Louise Despert), schizophrenia (Frieda Fromm-Reichmann), crippled and disabled (Bychowski), stutterers (Glauber), alcoholics (Ruth Fox), sex offenders (Abrahamsen), psychosomatic medicine (Melitta Sperling), borderline cases (Eisenstein), depression (Bellak), and Rorschach (Piotrowski and Schreiber). Index.

** FOULKES, S. H. AND ANTHONY, E. J. *Group Psychotherapy*. (A 370.) Penguin, 1957. Pp. 263. \$85. Original.

This is a splendid book on group psychotherapy by two British psychiatrists. It covers all phases of the subject, including practice and theory. The high level of communication is characteristic of Penguin originals. Index.

* MAY, R. *The Art of Counseling*. (Apex B 6.) Abingdon Press, n.d. Pp. 247. \$1.25. Original.

This book could be more accurately titled: *The Art of Pastoral (or Religious) Counseling*. It is intended primarily for ministers and divinity students. Alfred Adler is its *elan vital*. Index.

General and Theoretical Psychology

** EYSENCK, H. J. *Sense and Nonsense in Psychology*. (A 385.) Penguin, 1957. Pp. 349. \$85. Original.

I cannot be objective about this book because one of the chapters, *The Interpretation of Dreams*, contains large amounts of material that seem to be paraphrased from my book, *The Meaning of Dreams*. I wonder if the seven other chapters on hypnosis, lie detection, telepathy, measurement of personality, personality and conditioning, politics and personality, and the psychology of esthetics are similarly contrived. It is a sprightly book that can be used advantageously in the first course. Index.

* HARRIMAN, P. L. *An Outline of Modern Psychology*. Littlefield, Adams, 1956. Pp. 286. \$1.50. Original.

The books on psychology in this or other outline series are paperbacks that serve a specific purpose; to help students review a subject. Apparently they are used for this purpose. Self-testing exercises enable the student to test his knowledge. A great amount of factual material and names galore (is it really necessary for a student to know so many names?) are compressed into a relatively short book. There are also outlines of abnormal and educational psychology in this series.

** HAVEMANN, E. *The Age of Psychology*. Simon and Schuster. \$1.00.

Not seen.

** HUNTER, I. M. L. *Memory: Facts and Fallacies*. (Pelican A405.) Penguin, 1958. Pp. 185. \$85. Original.

Hunter, a Lecturer in Psychology at the University of Edinburgh, has written an up-to-date account of memory which is firmly based on psychological research. It is an extremely valuable addition to the growing list of paperback books that can be used as supplementary readings in the beginning course. Index.

* JAMES, W. *The Principles of Psychology*. (T381 and T382.) Dover, 1950. Volume 1. Pp. 689. \$2.00. Volume 2. Pp. 688 + index. \$2.00. Unabridged. Reprint.

The famous long course, complete and unabridged.

* OGDEN, C. K. AND RICHARDS, I. A. *The Meaning of Meaning*. (Harvest.) Harcourt, Brace. \$2.25.

Not seen.

* SCHRÖDINGER, E. C. *Science Theory and Man*. (T428.) Dover, 1958. Pp. 223. \$1.35. Reprinted and original essays.

Psychologists will enjoy reading many of these essays because they grapple with fundamental epistemological problems common to all of the sciences. Schrödinger, who won the Nobel prize in 1933 for his work on wave mechanics, writes with great clarity about abstruse matters. He uses familiar analogies to exemplify difficult concepts. No index.

*** SPERLING, A. P. *How to Make Psychology Work for You*. (Premier s48.) Fawcett, 1957. Pp. 192. \$35. (Originally published under the title *Psychology for the Millions* but revised for this edition.)

This book is an effervescent mixture of anecdotes, witticisms, homespun philosophy, moralizing, popularizations of serious psychological studies, and titillating references to sex—all of which goes to prove that, if you cheapen psychology for the masses, you merely make it worthless. I

see no use for such a book as this is, in or out of the classroom.

** SPERLING, A. P. *Psychology Made Simple*. Made Simple Books, 1957. Pp. 192. \$1.00. Original.

Everything is being simplified today. This book is one in a series of *Made Simple* volumes ranging from *Chess Made Simple* to *Physics Made Simple*. Practically all of them are written by people with PhDs. Sperling has crammed a lot of psychology into 192 pages, most of it of the traditional sort. There are the usual chapters on sensation, perception, learning, motivation, emotions, personality, and the contents conform pretty much to the kind of psychology that most instructors teach their students. The suggestions for further reading at the end of each of the 17 chapters list the standard reference works. There is a glossary but no index. The book could be used in high school, extension, night school, adult education, technical or trade school, or in any other type of educational setting where simplicity and conciseness of subject matter are deemed essential. It is not a pocket book but measures 8 by 10½ inches. The type is large, the paper is cheap, and the cover is sober. It is a far better book than the other one by Sperling listed above. What would you say about psychology if you had only 150,000 words in which to say it?

* TOLMAN, E. C. *Behavior and Psychological Man*. University of California Press, 1958. Pp. 269. \$1.95. Unabridged. Reprint of *Collected Papers in Psychology*.

In 1951, a group of students and colleagues honored Edward Tolman by sponsoring the publication of a volume of his papers. Tolman, himself, made a selection of 19 articles from a bibliography of over eighty items. The collection spans the years from 1922, *A New Formula for Behaviorism*, to 1948, *Cognitive Maps in Rats and Men*. Next to his *Purposive Behaviorism*, this is Tolman's most important statement of his theory. Two threads run through the maze of Tolman's thinking: purpose and cognition. No index.

* WATSON, J. B. *Behaviorism*. (Phoenix P23.) University of Chicago Press. \$1.50.

Not seen.

Personality and Abnormal Psychology

* ABRAHAMSEN, D. *Who Are the Guilty?* (Evergreen E-106.) Grove, n.d. Pp. 340. \$1.95. Unabridged. Reprint.

This is an informally written book by a psychiatrist about criminals. It contains a number of case histories. Index.

*** FREEMAN, LUCY. *Catch Me Before I Kill More*. (C-221.) Pocket Books, 1956. Pp. 357. \$.35. Unabridged. Reprint.

A readable account of the case of William Heirens, the University of Chicago student, who murdered three women. It is a well-documented presentation and is a valuable addition to the literature of case studies. No index.

* MCCARY, J. L. (Ed.) *Psychology of Personality*. (Evergreen, E-139.) Grove, 1959. Pp. 383. \$1.95. Unabridged. Reprint.

In 1954, a symposium on personality was held at the University of Houston. Bellak writes on psychoanalytic theory, Cattell on structural measurement, Klein on perception and personality, Mead on a cross-cultural approach, and N. Sanford on the authoritarian personality. McClelland attempts to give an integrative view. It strikes me that this symposium is better than most. Psychologists are doing a lot of traveling on the symposium circuit these days. The favorite speakers can sometimes be heard saying the same thing again and again. Index.

* PRINCE, M. *The Dissociation of Personality*. (MG 12.) Meridian, 1957. Pp. 575. \$1.95. Unabridged. Reprint.

In the same year (1898) that Dora made her initial visit to Freud, Sally Beauchamp knocked on the consulting room door of Morton Prince. Both young ladies subsequently became famous in the annals of psychiatric case histories. Dora suffered from hysteria, Sally was a case of multiple or disintegrated personality. This book was first published in 1905, as was Freud's Dora. It had a second edition in 1908. The Meridian edition is a reprint of the second edition. It is good to have this 'classic' available in a relatively inexpensive edition, and it deserves to be widely used in courses in abnormal psychology. Index.

Psychoanalysis

*** ADLER, A. *Understanding Human Nature*. (Premier, D52.) Fawcett, \$.50. Not seen.

* BERNE, E. A. *Layman's Guide to Psychiatry and Psychoanalysis*. Simon and Schuster, 1957. Pp. 321. \$1.50. (Revised edition of *The Mind in Action*.)

Dr. Berne, a psychoanalytic psychiatrist, says that "the object of this book is to make the dynamics of the human mind intelligible to those who are more interested in understanding nature than in using big

words or memorizing definitions." He has succeeded very well. Good bibliographies, glossary, and index.

** BRENNER, C. *An Elementary Textbook of Psychoanalysis*. (Anchor A 102.) Doubleday, 1957. Pp. 224. \$.95. Unabridged. Reprint.

Just what the title says it is. The emphasis throughout is on normal psychological processes, but there is a chapter on psychopathology. None on psychotherapy, however. The author is a psychoanalyst and the book is based upon lectures he has given to psychiatric residents and medical students. It is clearly written and should not be above the heads of undergraduates. Recent developments in ego theory are included. Index.

** BRILL, A. A. *Lectures on Psychoanalytic Psychiatry*. (Vintage K11.) Knopf, 1955. Pp. 302. \$.95. Unabridged. Reprint.

A. A. Brill was for years Freud's ambassador in the United States. These informal and discursive lectures on psychoanalysis were given annually for a number of years at the New York State Psychiatric Institute. There are replete with the author's personal experiences and cases. Each of the ten lectures, with several exceptions, is followed by a short question-and-answer period. Index.

* CLARK, R. A. *Six Talks on Jung's Psychology*. Boxwood, 1953. \$1.50. Pp. 84. Unabridged. Original.

The topics covered by these six elementary talks are libido theory, psychological types, the shadow, archetypes, dreams, and psychotherapy. The author is an American psychiatrist who has studied with Jung in Zurich. For an introduction to Jung, the Frieda Fordham volume is to be preferred. (See CP, Feb. 1956, I, p. 293.) No index.

*** EIDELBERG, L. *Take Off Your Mask*. (R273.) Pyramid, 1957. Pp. 157. \$.35. Reprint.

This book does a disservice to psychoanalysis. The psychoanalyst, a Dr. Rodan in the book, behaves as no psychoanalyst would, and his patients are caricatures. We are told that the author "extracted certain pertinent facts from the cases of many patients, my own and others, and have attempted to coalesce them into the figures who move through this book." The figures are too wooden to move anywhere.

* FERENCZI, S. *Sex in Psychoanalysis* and FERENCZI, S. and RANK, O. *The Development of Psychoanalysis*. (T324.) Dover, 1956. Pp. 288 and Pp. 68. \$1.85. Unabridged. Reprint.

Sex in Psychoanalysis is a reprint of Ferenczi's *Contributions to Psycho-Analy-*

sis, which is a collection of articles published between 1908 and 1914. These articles established Ferenczi as one of Freud's most creative colleagues. *The Development of Psychoanalysis* played a significant part in the history of psychoanalysis. (See Ernest Jones' third volume on Freud.) It marked the beginning of Rank's defection, and Ferenczi almost left the movement with him. The ties of affection between Freud and Ferenczi were too strong, however, and Ferenczi remained in the inner circle.

** FREUD, S. *Civilization and Its Discontents*. (Anchor A130.) Doubleday. \$.95. Not seen.

* FREUD, S. *Delusion and Dream*. (BP19.) Beacon. \$1.45. Not seen.

** FREUD, S. *Moses and Monotheism*. (Vintage K-14.) Knopf, 1955. Pp. 182. \$.95. Unabridged. Reprint.

Written when Freud was over eighty years old, this astonishing book treats of the origins of the Jewish religion and of religion in general. Throughout the book there are many discussions of psychoanalytic concepts, although no new ones are introduced and little fresh light is cast upon the old ones. The cover design is particularly attractive. Recommended for courses in the psychology of religion. Index.

* FREUD, S. *On Creativity and the Unconscious*. (Torchbooks TB 45.) Harper, 1958. Pp. 310. \$1.85. Unabridged. Reprint.

The essays in this volume originally appeared in the fourth volume of Freud's *Collected Papers* under the title, *Papers on Applied Psychoanalysis*. Index.

** FREUD, S. *The Future of an Illusion*. (Anchor A99.) Doubleday, 1957. Pp. 102. \$.95. Unabridged. Reprint.

Gradually, Freud's extensive writings are appearing in paperback editions. This well-known essay on the psychology of religion (religion is the "illusion") is the ninth of Freud's books to become available in paperback. The cover design and typography are among the finest I have seen for a paperback book. It almost approaches the elegance of Japanese paperbacks. But it is overpriced. For courses in the psychology of religion but also of general interest. No index.

* FREUD, S. *The Origins of Psychoanalysis: Letters, Drafts and Notes to Wilhelm Fliess (1887-1902)*. (Anchor A112.) Doubleday, 1957. Pp. 384. \$1.25. Abridged. Reprint.

The hardcover edition of this book, published in 1954, sells for \$6.75 or more than

five times the price of this edition. Those of us who bought the hardcover edition through Basic Books may be annoyed that we did not wait several years for the inexpensive one. But wait—something is missing. *The Project for a Scientific Psychology*, a hundred-page essay which Freud wrote in 1895 and which has not hitherto been published in English, has been omitted from this Anchor edition. Some consider this the most important document in the *Origins*. Kris's valuable introduction has been retained, as have the fascinating letters of Freud to Fliess. This book is for the serious student of Freudian psychology and its usefulness in the classroom is limited to advanced graduate courses in the history of psychology and psychoanalysis. Index.

* MENNINGER, K. *Love Against Hate*. (Harvest.) Harcourt, Brace. \$1.95. Not seen.

* MENNINGER, K. *Man Against Himself*. (Harvest.) Harcourt, Brace. \$1.45. Not seen.

* MULLAHY, P. (Ed.) *A Study of Interpersonal Relations*. (Evergreen E-76.) Grove, 1957. Pp. 507. \$1.95. Unabridged. Reprint.

With the reprinting of this collection of articles from the journal, *Psychiatry*, the Washington School of Psychiatry is now represented in the paperbound library of psychology books. There are articles by the leader and many of his disciples. Clara Thompson writes on women and homosexuality, Frieda Fromm-Reichmann on therapy, Shachtel on memory and amnesia, Hutchinson on insight and creativity, and lots of other people write on lots of other subjects. Mullahy contributes a sketchy introduction. If you want your students to be familiar with the kind of article that appears in the house-organ of the Washington School, this is the book for you. No index.

* NELSON, B. (Ed.) *Freud and the 20th Century*. (M 45.) Meridian, 1957. Pp. 314. \$1.45. Unabridged. Original and reprinted articles.

It will serve a useful purpose to list the contents of this volume, cite the original sources of the articles, and say a word about the contributors where necessary. Kazin's *The Freudian Revolution Analyzed* appeared in the *New York Times Magazine*, May 6, 1956. He is a professor of literature. Walker's *A New Copernicus?* is reprinted from the *Listener*. Walker is an Edinburgh psychologist. Zilboorg's *The Changing Concept of Man in Present-Day Psychiatry* is reprinted from the *American Journal of Psychiatry*. Freud's *A Preface: 1917* is a preface to Reik's *Ritual*. Kar-

diner's *Freud: The Man I Knew, the Scientist and his Influence* is an original essay prepared for this volume. *Reminiscences of Freud and Jung* consists of translated excerpts from von Weizsaecker's *Natur und Geist* (1954). He is a German psychoanalyst and philosopher. Erikson's *The First Psychoanalyst* was originally published in the *Yale Review*, September 1956. *The Current Impact of Freud on American Psychology* is Gardner Murphy's address to the Division of Clinical Psychology in Chicago, August 30, 1956. Hacker's *Freud, Marx, and Kierkegaard* was prepared for this volume. Hacker is a psychiatrist. *Freud, the Revisionists, and Social Reality* by Will Herberg, a professor of Judaic Studies at Drew University is an original essay. Hyman's *Psychoanalysis and the Climate of Tragedy* first appeared in the *Partisan Review*, Spring 1956. Hyman is a teacher of literature at Bennington College and a staff writer for the *New Yorker*. Gombrich's *Psychoanalysis and the History of Art* is reprinted from the *International Journal of Psycho-analysis*. Gombrich is a professor of art. *Freud and Modern Philosophy: A Thomist View* is a revision of an essay which appeared in *Scholasticism and Politics*. *Human Creativity and Self-Concern in Freud's Thought* was written for this volume by Reinhold Niebuhr. Jerome Bruner's *Freud and the Image of Man* first appeared in the *Partisan Review*, Summer 1956. A splendid collection of essays. No index.

* REIK, T. *Masochism in Modern Man*. (Evergreen E-79.) Grove, 1957. Pp. 439. \$1.95. Unabridged. Reprint.

If there is anything which has been learned about masochism since the Austrian novelist, Leopold von Sacher-Masoch (1835-1895) first described the condition and which has been omitted from this exhaustive study, it is probably not worth knowing. *Joy Out of Suffering*, the title of the German edition, or victory through defeat—this is the aim of masochism according to Reik. The book is beautifully written by one of the wisest heads in psychoanalysis. For courses in abnormal psychology. Index.

* REIK, T. *Of Love and Lust*. (Evergreen E-135.) Grove, n.d. Pp. 623. \$2.45. Reprinted and original selections.

"*Of Love and Lust* is the second of a series of volumes of selections from Theodor Reik's work. In this new volume he is concerned with the love life and the sexual life of men and women. Part One is taken

from one of his most successful books, *A Psychologist Looks at Love*, out of print now for some years. Part Two is from Reik's great contribution to psychological literature, *Masochism in Modern Man*. Part Three, *The Unmarried*, consists of two essays, written for the symposium *Why Are You Single*, edited by Hilda Holland. Part Four, *The Emotional Differences of the Sexes*, is the new and unpublished material." (From the Publisher's Preface, by John Farrar.) No index.

* REIK, T. *The Search Within*. (Evergreen E-107.) Grove, n.d. Pp. 657. \$2.95. Reprinted and original selections.

"*The Search Within* is designed as the first of a series of volumes of selections from Theodor Reik's works. This initial volume is a synthesis of his frank reminiscences of his personal life, his training, practice and the development of his philosophy. Part One is taken from his memories of days with Freud, originally published under the title, *From Thirty Years with Freud*, except for the essays on *Conversations with Freud and Jewish Wit*, which have not been previously published. Part Two, *The Confessions of an Analyst*, is winnowed from *Fragments of a Great Confession*. Part Three, *The Gift for Psychological Observation*, is a short selection from *Listening with the Third Ear*. Part Four, *Psychoanalytic Experiences in Life, Literature and Music*, is drawn from *The Secret Self and The Haunting Melody*, and extends some of these discoveries. Part Five, *Adventures in Psychoanalytic Discovery*, is the lately written and hitherto unpublished material." (From the Publisher's Preface, by John Farrar.) No index.

* RICKMAN, J. *A General Selection from the Works of Sigmund Freud*. (Anchor A115.) Doubleday, 1957. Pp. 295. \$1.25. Unabridged. Reprint.

This selection has been made primarily from Freud's writings on psychological theory. The important articles published during the war years (e.g., *Instincts and Their Vicissitudes*, *Repression*, *On Narcissism*, *Mourning and Melancholia*) and the monographs of the early twenties (e.g., *Beyond the Pleasure Principle*, *Group Psychology and the Analysis of the Ego*, *The Ego and the Id*) are well represented. There is one serious drawback to the present anthology; most of the articles and monographs are not reprinted in their entirety but consist of excerpts. Since these theoretical writings of Freud are closely reasoned and must be read with careful attention to his subtle line of argument, they cannot stand much abridgement. For example, *Beyond the Pleasure Principle*, which is considered to be the most abstruse

essay Freud ever wrote, is reduced by half in this anthology. His other writings are similarly compressed. It is, therefore, difficult to see what value this selection possesses for students of psychology. Index and glossary.

* THOMPSON, CLARA with the collaboration of P. MULLAHEY. *Psychoanalysis: Evolution and Development*. (Evergreen E-67.) Grove, 1957. Pp. 252. \$1.75. Unabridged. Reprint.

This account of psychoanalysis is written by a proponent of the interpersonal theory of Harry Stack Sullivan. Index.

Religion and Psychology

* HERBERG, W. (Ed.) *Four Existentialist Theologians*. (Anchor A141.) Doubleday, 1958. Pp. 312. \$1.25. Reprinted selections.

The four existential theologians are Maritain, Berdyaev, Buber, and Tillich. No index.

*** MURPHY, CAROL. *Religion and Mental Illness*. (No. 82.) Pendle Hill, 1955. Pp. 31. \$.35. Original.

A Quaker examines the relevance of religion for dealing with the mentally ill and concludes that "the mentally ill can lead us back to a truer and deeper service to all mankind."

*** OATES, W. E. *What Psychology Says about Religion*. Association, 1958. Pp. 128. \$.50. Original.

Professor Oates of the Southern Baptist Theological Seminary detects four "postures" that psychology has held towards religion. It has remained silent (Pavlov), it has rejected religion (Watson, Freud), it has displayed a critical but cautiously affectionate attitude (Allport, Fromm, Menninger), and it has discovered the "living beating pulse of spirituality as it is psychologically perceived" (Jung, Adler). The copyright on this tract is held by the YMCA. Index.

* RADIN, P. *Primitive Religion*. (T293.) Dover, 1957. Pp. 322. \$1.85. Unabridged. Reprint.

"Perhaps it might be well to restate what I had in view in my book. It was first of all a protest against the manner in which these religions had been described by the majority of anthropologists, missionaries, administrators and students of comparative religion. Secondly, it was an attempt to examine carefully three specific aspects of these religions: the nature of the religious experience itself, the varying degree to which individuals possessed it, and lastly, the role played by social-economic forces and the extent to which the latter fashioned and directed the basic ex-

pressions religion had assumed in the aboriginal world." (From Paul Radin's Preface, written in 1956 for this reprint.) Index.

Social Psychology

* ALLFORT, G. W. *The Nature of Prejudice*. (Anchor A149.) Doubleday, 1958. Pp. 496. \$1.45. Abridged by the author. Reprint.

This is the finest introduction to an understanding of prejudice with which I am familiar. It is a book conceived in wisdom and nurtured by research. In a foreword for this edition, Allport criticizes the Supreme Court for its policy of 'deliberate speed.' He believes that school integration should have been put into effect quickly before the opposition had a chance to consolidate its forces. Now that a gradualist position has been adopted, Allport recommends that integration begin in the elementary schools first. Index.

* BERNARD, JESSIE, BUCHANAN, HELEN E., AND SMITH, W. M., JR. *Dating, Mating, and Marriage*. Allen, 1958. Pp. 410. \$3.50. Original.

A large part of this book consists of cases and documents, many of them written by students. The introduction to each chapter establishes a systematic setting for the case material that follows. It can be used as the principal textbook in a marriage course or in conjunction with a standard text. The cases and documents are keyed to all of the current textbooks in the area. It is well written and should prove to be immensely popular with students. Index.

*** PACKARD, V. *The Human Persuaders*. (C288.) Pocket Books, \$.35.

Not seen.

Miscellaneous

*** ARNOLD, PAULINE. *Rate Yourself*. (M-4084.) Permabooks, 1958. Pp. 165. \$.35. Original.

For people who like to take tests and solve problems.

*** DINGWALL, E. J. *The American Woman*. (Signet D1591.) New American Library, 1958. Pp. 288. \$.50. Unabridged. Reprint.

This is another one of those books—this one by a British anthropologist (University of London PhD). If you hate women, this book is your dish of tea. Anyone can select facts to prove almost anything. Index.

* GARDNER, M. *Fads and Fallacies*. (T394.) Dover, 1957. Pp. 363. \$1.50. Original. (Revised edition of *In the Name of Science*, 1952.)

If you are an admirer of any of the following men and movements—Bates' sight without glasses, Reich's orgonomy, dianetics, general semantics, Moreno's psychodrama, ESP, or Bridey Murphy—don't raise your blood pressure by reading this iconoclastic book. If you are a skeptic you will find considerable evidence to support your skepticism and arouse skepticism in your students. The author is not identified beyond his name. He is a free-lance writer and, in connection with another book, he is called an expert on mathematical magic. He is witty and sophisticated, and acknowledges having received help from the Editor of *CP*. Annotated and indexed.

*** KLING, S. G. *How to Win and Hold a Mate*. (M-4074.) Permabooks, 1957. Pp. 245. \$.35. Original.

An advice book by a marriage counselor and attorney. Written in an easy-to-read question-and-answer form. Some of the answers are based upon scientific data, others are primarily anecdotal. Not particularly useful for college courses. No index.

* LUCAS, F. L. *Literature and Psychology*. (AA11.) University of Michigan Press, 1957. Pp. 340. \$1.75. First American Edition Revised.

This is a chatty series of lectures by an English professor with a well-stocked mind regarding literature. One will find precious little psychology in the book and much of what is there has been learned from Stekel. The author is a healthy Britisher who is fascinated by case histories but repelled by "decadent" literature (e.g., Poe and Proust). A bridge between psychology and literature has yet to be erected. I cannot see that this book would serve any useful purpose in courses in psychology. Index.

** WIENER, N. *The Human Use of Human Beings*. (Anchor A34.) Doubleday. \$.75. Not seen.

Addresses of Publishers

- Abingdon Press, 201 8th Ave., S., Nashville 2.
- Howard Allen, Inc., Box 1810, University Center Station, Cleveland 6.
- Association Press, 291 Broadway, N. Y. 7.
- Beacon Press, 25 Beacon St., Boston 8.
- The Boxwood Press, P. O. Box 7171, Pittsburgh 13.
- Dell Publishing Co., 261 Fifth Avenue, N. Y. 16.
- Doubleday and Co., 575 Madison Ave., N. Y. 22.
- Dover Publications, 920 Broadway, N. Y. 10.
- Fawcett Publications, 67 W. 44 St., N. Y. 36.

Grove Press, 795 Broadway, N. Y. 3.
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 N. Y. 17.
 Harper & Brothers, 49 E. 33 St., N. Y. 16.
 Alfred A. Knopf, 501 Madison Ave., N. Y.
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 Littlefield, Adams & Co., 128 Oliver St.,
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 Made Simple Books distributed by Double-
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 N. Y. 22.

Pendle Hill Pamphlets, Wallingford, Pa.
 Penguin Books, 3300 Clipper Mill Rd.,
 Baltimore 11.
 Permabooks. See Pocket Books.
 Pocket Books, 630 Fifth Ave., N. Y. 20.
 Pyramid Books, 444 Madison Ave., N. Y.
 22.
 Simon and Schuster, 630 Fifth Ave., N. Y.
 20.
 University of California Press, Berkeley 4.
 University of Chicago Press, 5750 Ellis
 Ave., Chicago 37.
 University of Michigan Press, Ann Arbor,
 Mich.

The Ego at Work in India

Albert Mayer and Associates, in collaboration with McKim Marriott
 and Richard L. Park

Pilot Project, India. (Foreword by Pandit Govind Ballabh Pant.) Berkeley
 and Los Angeles: University of California Press, 1958. Pp. xvii + 367.
 \$5.50.

Reviewed by GARDNER MURPHY and LOIS BARCLAY MURPHY

Gardner and Lois Murphy are both well known as psychologists and as a team. They married each other in 1926 and each has a PhD from Columbia, though at quite different dates. With T. M. Newcomb they published Experimental Social Psychology, revised edition, in 1937. Gardner is known for many books: Historical Introduction to Modern Psychology (1929, 1949), Introduction to Psychology (1951), and recently Human Potentialities (1958; CP, June 1959, 4, 161-164). Lois, for many years at Sarah Lawrence College, is known for Personality in Young Children (1956; CP, Mar. 1957, 2, 69f.). They were together in India in 1950 and 1955. Lois was helping to found an institute of child development at Ahmedabad, and Gardner was studying India as a social problem. They are both in Topeka now with The Menninger Foundation.

THIS volume is at the junction of two main highways of modern social-science research. (1) One of these roads marks the development of the experimental study of the community, in the sense that approaches to community life are constantly reformulated

and tried out, changes in operating procedure effected, records kept, hypotheses restructured, and experimental refinements perfected. While constantly assessing on-the-spot realities, all of this work represents the best tradition in the action research of Kurt Lewin. The task to be done dictates the working concepts, the methods, the types of reports drawn up, the types of conclusions and applications formulated. (2) The other road represents a more general movement in social science toward the clarification of concepts as to what goes on in a scrambled and bewildering social situation, in which experimental intervention is possible only on a very limited scale, yet in which there is an achievement of some sort of broad, human generalizations that go beyond the local and particular needs to be met. We think Lewin, who gave the earliest years of his professional life to a struggle in the direction of mathematical simplicity, clarification, and order, might be distressed at our implication that action research as such could not ordinarily achieve broad clarification and order but must limit itself to the attempt to derive local and particular

working conclusions, to be plowed back into more local and particular research efforts. There seems, however, to be coming into existence a type of social research in which wide human generalizations can be experimentally tested. This book by Albert Mayer exemplifies the trend.

When we talked to Mayer in Bombay in 1950, we felt that his tough realistic approach as an architect and city planner, and his sensitive, subtle perception of the enormous capacity of modern India to use generous, imaginative methods of self-education and self-development, could give a very rich orchestration of new ideas. India was proving to be the natural habitat for a down-to-earth practical planner, not only because of the desperateness of its need, the seething demand of the hungry and bewildered population, but also because of the availability of a trained 'village-level leadership.' There was likewise a general readiness to accept lower-level and middle-level types of activity that had long been underplayed in the British regime. And, finally, there was a sensitization everywhere to the critical importance of acting because of the fierce light that beats upon a really new situation in which a country suddenly finds itself (as it may once in a thousand years) ready to do something fundamentally, heroically new.

The present volume tells how Mayer and the top-level, middle-level, and lower-level leaders with whom he worked found a dynamic way in which to increase enormously the agricultural yield of a large region in northern India, while they were, at the same time, providing vast increments of education, applied science, and sheer self-respect to the millions of simple people who were ready for—they did not know quite what until they saw it taking all these forms, themselves giving more than they knew they were capable of giving. For example, we saw there a new road that had been made at night on village initiative after the villagers had spent the day on tasks agreed upon with the planner.

THIS volume explains how it all began in 1946, when Mayer, after war service,

had the chance to talk to Premier Pant of the United Provinces in north central India, and then with Pandit Nehru himself (who, though he had no time from dawn to midnight, could still talk for a few minutes with Mayer every night as he got ready to go to sleep). Such talks convinced Nehru that the originally contemplated "model village" would be of no use, and that the thing to do was to work through with village people themselves the pressing problems of ways of increasing agricultural yield, improving sanitation, and so on. Soon it became clear that everything had to be done at once.

Mayer and his American collaborators here fit together their memoranda, interim reports and retrospective reflections, to give vivid pictures of the project from 1946 to the present. All the time they were immersed in many concrete and grubby realities. Better seed, better food for the animals and better veterinary care, better wells, better communications between villages, better schools, better adult-education programs, all had to be thought through in a staggered or even a simultaneous co-working program with well-trained, multi-purpose 'village-level workers' brought in and helped to understand local village problems. Along with the Mayer staff itself, they were ready to get dirty and exhausted in bucking the most formidable of the problems, without waiting for the administrator to tell the villagers what to do. The records, kept constantly, made it possible to say afterwards just what targets had actually been achieved and at what dates. The records showed that the objective gains could be used to reduce the tax burden which had been willingly assumed by the local and more general public, so that more and more tax support could be made available for more and more ambitious expanding projects.

Does it sound easy? One must remember that the Indian villager had always been told what he ought to do. Over and over again, the narrative reminds one how wells had been built and abandoned, how tools and tractors had been imported and fallen into rust and disuse. In the new approach each step had to validate itself, and that could be

done by vigilance, by careful watching for signs of faith and despair, not only by the local elected leaders (the members of Panchayats) but also by cultivators themselves.

Now what 'generalizations' does this volume yield? The most extraordinary thing in this extraordinary book is the sensitive essay it writes on 'ego-involvement' and 'group-involvement' without ever using these words, and the way in which these principles are thrown into

relief as general psychological principles, with an even greater clarity than has been achieved in the experimental 'small group research' of the West. People had to be shown that the problems were *theirs* and that they were *soluble*. The traditional teaching methods had been failing right and left, but now a practical democracy, instead of being preached, was being ocularly exhibited, day by day, as fully capable of getting the necessary jobs done.

Professors under the Microscope

Paul F. Lazarsfeld and Wagner Thielens, Jr.

The Academic Mind: Social Scientists in a Time of Crisis. (With a Field Report by David Riesman.) Glencoe, Ill.: Free Press, 1958. Pp. xiv + 460. \$7.50.

Reviewed by DONALD G. PATERSON

In World War I Dr. Paterson was psychological examiner at six different Army camps and after that one of the collators of the data of that enterprise. He spent a couple of years working on testing with the Scott Company, but ever since 1921 he has been Professor of Psychology at the University of Minnesota, where he has been busy with occupational psychology, vocational psychology, questionnaire methods, interviewing procedures, psychometrics, and individual differences. He is the author of many books, from the Scale of Performance Tests (1917) to the Revised Minnesota Occupational Rating Scales (1953). He was editor of the Journal of Applied Psychology from 1943 to 1954.

THIS is an exciting book. It attempts to reveal the reactions of 2,451 social scientists in 165 institutions of higher learning to the menace of what many people call *McCarthyism*. The book itself deals with these reactions in the Spring of 1955 and is primarily concerned with the impact of events of the preceding six or seven years. These are designated "the difficult years" for American intellectuals either in 'town' or 'gown.'

Fully to appreciate the significance of the findings reported, the reader would

do well to read or re-read the study by Samuel A. Stouffer and his associates in Harvard University's Laboratory of Social Relations which recorded the reactions of a national cross-section of adults and selected community leaders toward Communism and related issues during the summer of 1954 (*Communism, Conformity, and Civil Liberties*, Doubleday, 1955). Both books deal with similar issues. Both exhibit polling techniques and the reporting of results at the highest level of competence so far achieved in nation-wide studies.

The result of both books? A partial answer to Robert Burns' classic plea:

O wad some Pow'r the giftie gie us
To see oursels as others see us!

CP (*Contemporary Psychology* and *not Communist Party!*) insists that a reviewer tell its readers something about the authors. This reviewer first met Lazarsfeld when he came to America from Vienna in the late 1930s as a young, eager, brilliant psychologist who was insatiably curious about everything going on in our psychological laboratories and in America as well. Since then he has made outstanding contributions to the field of sociology and social psychology, especially its subfield of opinion measurement, and he now heads the Bureau of Applied Social Re-

search at Columbia University. (Curiously enough, Lazarsfeld, who thrives on the willingness of respondents to comply with probing interviews and questionnaires, fails to answer the pleas of the American Psychological Association for information about himself for its Directory!) Thielens is a Lecturer in Sociology at Columbia University and Research Associate in its Bureau of Applied Social Research.

The authors drew up the extensive interview schedule, made the analysis, and reported the results. Two organizations (Elmo Roper and Associates, and the National Opinion Research Center) managed the sampling and the interviewing. David Riesman, an eminent sociologist, contributed a unique feature—a follow-up study of the interviewees and interviewers. His report makes interesting reading and provides grist for the mill of the purists and perfectionists who are wont to discredit all polls and pollsters. It also provides reassurance for the applied scientist that this particular job, with all its imperfections, was done with integrity and competence.

By restricting the study to social scientists the authors simplified their task to a degree; but only to a slight degree, because higher education in America is an extraordinarily heterogeneous collection of institutions with respect to size, control, objectives, and quality.

The demographic and ecological facts reported about the respondents and their institutions as given in Chapter I were of special interest to this reviewer, and he believes they will be appreciated by all academicians as well as by those segments of the public interested in American higher education.

Less competent analysts would have become lost in the maze of detailed facts provided by the heterogeneity of the respondents; but Lazarsfeld and Thielens have skillfully marshalled the evidence to tell a coherent and fascinating story as to patterns of apprehension and action exhibited by the academic targets of charges, accusations, exposure techniques, and character assassinations, let loose by powerful individuals and groups in America who were over-reacting to the menace of Communism. The picture emerges from

the reports of 990 separate incidents occurring in the 165 institutions. The number of separate incidents for any one school ranged from 0 to 28, with no incidents reported for 24 colleges (almost all of these among the smallest). Concentration of the incidents was in the larger secular colleges and universities (both private and public). A heavy concentration of these incidents involved political issues but were by no means confined to this area, for many of the incidents concerned religion, segregation, economics, personal idiosyncrasy, immorality, unusual teaching techniques, restrictions imposed by authority (gag rules about campus speakers, limitations on research, etc.), and loyalty oaths. The count of incidents itself was so conservative that it is clear that the reverberations and the numbers of people involved directly or indirectly on the campuses of America far exceeded the mean of 6.0 incidents per college or university.

It is unfortunate that the study was confined to social scientists because many of the charges aimed at colleges and faculties were directed not at this group but at physicists, chemists, philosophers, English teachers, law school professors, and others not included in the sample.

Highlights of the book include an ingenious index of apprehension for each respondent and type of school. A measure of the professor's willingness to take a stand on issues of academic freedom and an index of permissiveness which permits classification of each respondent on a 'permissive-conservative' continuum (in the reviewer's opinion the authors might better have called this a 'permissive-authoritarian' continuum) were also developed. Cross-tabulations show the relationship between these indices and a variety of reported activities (both classroom, intramural, and extramural) for different types of colleges and universities.

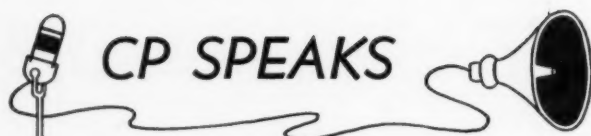
One limitation of the interview schedule lies in the fact that it attempted to probe only how the *individual* professor reacted to the situation. Had the schedule provided scope for discovering how professors reacted collectively, the picture of the academic mind would have been more complete. For example,

on one campus the reviewer knows of a series of defensive maneuvers that was undertaken with good effect. I refer to the strengthening and intensifying the work of the chapter of the American Association of University Professors, to the organization of a state affiliate of the American Civil Liberties Union in 1952, to the broadcasting of eight half-hour lectures on social science and freedom during the spring of 1955, and many other group activities designed to inform and alert the public. Professional and scientific organizations to which faculty members belong were also involved. The American Psychological Association, for example, set up a special committee to deal with the situation.

It is impossible to summarize the findings of the study in this review. The reader must go to the book to get even the main results, let alone the wealth of detail reported. The authors must have faced the same difficulty as the reviewer, since no summary chapter is provided for the busy reader. Perhaps that is good.

Over all, this reviewer believes that this book is a most important reference for present-day teachers and for those now contemplating teaching as a career. It is even more important that alumni, trustees, administrators, and community leaders read this account. They might in that way gain an understanding of the consequences that flow from irrational anti-intellectualism, unenlightened anti-communism, and all the other attempts to muzzle in colleges and universities the free inquiry so necessary to preserve a free society.

The reviewer wishes that the Fund for the Republic and other great foundations would subsidize similar studies of The Trustee Mind, of the College Administrative Mind, of the College Student Mind. These are urgently needed to round out the picture of these pressures. Such studies should be replicated at ten-year intervals to measure the extent to which free inquiry is being expanded or (perish the thought!) is being restricted in America. Furthermore such replication is needed because the pressures themselves keep shifting from one set of issues to another from decade to decade.



MALTHUSIAN MEN OF SCIENCE

IN July *CP* spoke of the problems created by splitting *American Men of Science*, first into three volumes (1955-56) and now into five (1959-62). Do you include the social scientists? Do you put the psychologists with them or with the biologists? Do you keep biological scientists separate from physical ones or combine them? (*CP*, 4, 204f.)

The trouble with *American Men of Science* is that it is getting too big. It has been increasing in coverage at 6% per annum since the first edition in 1906, and everybody knows that at 6% you can double your money in 12 years—your money or your scientists. Here is the picture:

Edition	Year	Approx. Entries	At 6% Increase
1	1906	4,000	4,000
2	1910	5,500	5,000
3	1921	9,500	9,600
4	1927	13,500	13,500
5	1933	22,000	19,300
6	1938	28,000	25,800
7	1944	34,000	36,600
8	1949	50,000	49,000
9	1956	91,000	73,700
10	1962	—	105,000
—	1975	—	200,000
—	1988	—	475,000
—	2000	—	950,000

In short, the five-volume tenth edition, now in preparation, bids fair to have well over 100,000 entries, and we shall be approaching the million mark by the year 2000, and—can it be possible?—32 volumes! That is an *eductio ad absurdum*! It would not be an *American Men of Science* any more, a volume that you keep around to look people up in. It would be a library reference work, and in the year 2000 it would cost about \$30 a volume, and for the 32 volumes the Library would pay \$1,000. That is

a function which *American Men of Science* was never intended to fulfill. If the present publisher continues on this path, then some other publisher will have to take over the responsibility he has abandoned.

You can say that populations do not go on increasing exponentially forever, that the growth curve straightens out in accordance with the population logistic, and indeed that may be true for American scientists. On the other hand, this actual curve takes a jump above the exponential one in 1956, suggesting that *CP*'s extrapolations may be too modest. Is *American Men of Science* becoming more democratic? Is it lowering its threshold?

What is wanted? *CP* says: A single, fat volume on tough thin paper with 50,000 entries, no more, spread all the way from physical to social science, with one alphabet. If the physicists want their own volume in addition, with more entries, they can have it. The psychologists have always had their own volumes. This one is for Science, not physics or psychology or biology or behavioral science.

Every list of this sort has a threshold. You do not put everybody in. You put only the more important people in. They are the people who are wanted most often. If it seems unfair or inadequate to leave the less important people out, then let someone get up a directory of the least important people. He will not succeed, for his directory will be too big and not used enough. The more important people are, by definition and by the psychology of attention, fewer than the less important. Let us use sense. Let us get back to what people want, what they will use most, 50,000 entries for *American Men of Science* in a single volume! The dinosaurs are dead. The elephants are diminishing as civilization

spreads. Gardner Murphy has come out against bigness (*CP*, July 1959, 4, 204). In this year of the Darwin centenary, let us favor intelligence, the survival of the fittest in *American Men of Science*. For the eleventh edition in 1968 why not get down to 50,000 scientific instances of *homo sapientissimus*? If the Editor does not know how to raise his threshold, *CP* feels sure that the psychologists, given the task, could work out the method for him.

SCIENCE AND THE VERNACULAR

RIGHTEOUSNESS is being good when it's hard, *CP* thinks. Telling the truth isn't righteous unless you want ever so much to tell something else. For the idealist who just knows how things ought to be and who is working his head off to make them so, for him opposition or even deviationism is wicked. No one is so intolerant as the would-be tolerant are of intolerance. And this bit of motivational dynamics gets into the judgment of books. When a book ignores your ideal, it is a bad book, even when it fulfills someone's else ideal, for its publication makes it harder for you to get ahead.

There's a book out called *What Makes You Tick?*—a congeries of forty bright quizzes by various authors, reprinted from *This Week Magazine* and edited by Charles D. Rice, with a foreword by W. J. E. Crissy, a Fellow of the American Psychological Association and a Diplomate in Industrial Psychology (Harpers, 1958, 229 pp., \$3.95). One member of the APA had denounced this book and suggested that *CP* review it, believing obviously that any good reviewer would pan it. So *CP* sent the book to a wise reviewer and he deplored it but said to skip it. Ordinarily *CP* would do just that: skip it. This time, however, *CP* is troubled by the protest. The book is largely just good clean fun. Its editor suggests that you take its quizzes to parties and get the crowd excited about *How to Find Out What Your Wife Thinks of You* or *How Bright Is Your Wife?* *CP* liked the *Seven Sure Ways to Win A Dollar*, because it sounded like Test 1 on the old Army Alpha Examination:

Announce to a friend that you'll bet him \$1 that you'll give him \$4 if he'll give you \$2.

No CP reader would be caught by that one, but it does make you pause, just as Army Alpha did:

If 7 is more than 5, then (when I say 'Go') cross out the number 6, unless 6 is more than 8, in which case draw a line under the number 7.

But, of course, if you are trying hard to get good solid psychological counseling across to the American public and *This Week Magazine* with its enormous circulation spoils your pitch by printing what is essentially a parody on sound psychological counsel, with a Diplomat in Industrial Psychology vouching for the fact that nothing in it is dead wrong, why then quite naturally you are angry, and you take out your spite by shrugging (cf. Charles Darwin on the shrug) and by saying: "But people are living beings; they don't tick!" Maybe not. Maybe the all-or-nothing action of the neuron is too fast to be a ticking. But this is America's language. Haven't psychologists enough to do without trying to alter the common speech?

Some can remember the public consternation when the data from the intelligence testing were given out after World War I. The shocked cry was: "Why almost half the country is feeble-minded!" Well, if it was, why then feeble-minded is just the old familiar state of every second man you meet. That's nothing new, nothing to worry about. It's the way the USA is.

There was another book twenty years ago, *We Call it Human Nature*, by Paul Grabbe, and sponsored by Gardner Murphy. (Also Harpers, 1939, 120 pp., and all full of pictures.) Murphy describes the pains and care he and the author took to develop this mode of getting attention and making learning easy for good solid psychological fact, but there was complaint about this book, some of it quite bitter and almost violent. Why? The book was sound enough and maybe it worked, though there is no story of its being a psychological best seller. But it was like a joke in a sermon. It is such hard work for psychologists to be right that they resent, some of them, the man who gives

the appearance of being right easily and gaily, no matter how excruciating it was to manufacture the gaiety.

There's been another instance of this deadening sobriety recently. A biologist writing in *Science* (5 Dec. 1958, 128, 1402-1405) is exhorting his fellows never to use teleological terms in their writings or lectures, to impress their pupils with the sin of the final cause. If you followed his advice literally, you really would never in a scientific context use the phrase *in order that*. Here is the sort of thing you must not say:

The insects have solved the problem of flight, which gives them immense advantages for dispersal, for seeking out appropriate growing places for their young.

Very little is known concerning these forces, except that they must be considerable in order to overcome the normal repulsion between the positively charged particles of the nucleus.

This antiteleologist feels very strongly about this matter. He is fighting purposivism and fighting's not joking. But have you got to wring all the verve out of the language in order to be scientific—to be as scientific as the Russians who do not, from this distance, seem to find science much fun? If we cannot have jollity in science, at least let us have a little urbanity. And let us not fight vernacularity; let us understand it and use it to get as much of the truth across to the subcollegiate stratum as we can. If *What Makes You Tick?* does not disseminate much truth, still it is about as harmless as wanting the Yankees to win (or the opposite).

BOOKS TO COME

FISH EGGS and elementary textbooks of psychology. Not all hatch and only the fittest survive—at least not all survive, and every now and then a psychopaleontologist turns up a fossil textbook that did survive and flourish long ago and then died, starved for love. James' *Principles* is the enormous exception. Unlike the fish eggs, no textbook was ever hatched without previous loving parental care in its fetal period, but father-love is not all, not enough to provide security against the ruthless ecology into which these didactic neonates emerge.

Every now and then CP hears of one of these undertakings which has had an especially long period of gestation, and which seems to promise, contrary to the biological rule, an extra dividend of maturity at birth. Here now is word about a book that may be called *Psychology*, parented by Donald G. Marquis and W. J. McKeachie, a text which Henry Holt might publish in the spring of 1961, after a whole dozen years of gestation. It is going to have a systematic integration and the instructor can not use the chapters in any order, not if he is sane. The warp will be human behavior and personality, and the woof biological, cultural, and social strands designed to provide a special pattern which is much more than an eclectic mixture. The two parents work together on the main design and then McKeachie tries out their expectations on Michigan undergraduates, who are said to survive. Better stick around until 1961. These authors are no unskilled craftsmen.

THERE'S a book due out in October by Frederick Herzberg, Bernard Mausner, and B. B. Snyderman that John Wiley is publishing and that is to be called *The Motivation to Work*. It is a follow-up of the 1957 volume called *Job Attitudes* by two of the same authors and two others (CP, July 1958, 3, 121f.). These are products of the Psychological Service of Pittsburgh. The forthcoming volume undertakes to strike the golden mean between the rigid results of mental tests on large numbers of subjects and the open-ended qualitative protocols of small groups. These authors have had 200 engineers and accountants say what in their job experiences made them happiest and what unhappiest. The two respective answers—says CP, translating a more elaborate description now in its hands—are goal-achievement and frustration. But read the book and see if CP's summary is right.



The man of science has learned to believe in justification not by faith but by verification.

—THOMAS HENRY HUXLEY

Delinquency's Origin: Two Approaches

F. Ivan Nye

Family Relationships and Delinquent Behavior. New York: John Wiley & Sons, 1958. Pp. xii + 168. \$4.95.

D. H. Stott

Unsettled Children and Their Families. New York: Philosophical Library, 1956. Pp. 240. \$6.00.

Reviewed by WILLIAM McCORD and ELEANOR MACCOBY

Dr. McCord is Assistant Professor of Sociology at Stanford University as well as Assistant Dean for Humanities and Sciences. He has been psychological counselor at two prisons: San Quentin and Norfolk, Mass. With his wife he is author of Psychopathy and Delinquency (1956) and Origins of Crime (1959).

Dr. Maccoby has been a lecturer in the Department of Social Relations and a research associate in the Laboratory of Human Development at Harvard University, but now she is moving to Stanford University as Associate Professor of Psychology. She is interested in the effects of the processes of socialization on juvenile delinquency, and of the role of the movies and TV and of parental beliefs and attitudes on socialization.

THE work of both Nye and Stott is addressed to a socially significant issue: the effect of familial relationships upon childhood deviance. They have, however, chosen dissimilar methods for analyzing the problem. Nye, a sociologist at Washington State College, examined the topic through a large statistical study (so common in American social science); Stott, a research fellow at the University of Bristol, prefers a 'typically' British approach—close analysis of individual case studies. The results of their researches reveal both the virtues and the deficiencies of these tactics.

Ivan Nye's work attempts to avoid the faults inherent in many analyses of juvenile delinquency, particularly the bias which comes from the researcher's dependence on institutionalization as an indicator of crime. He rightly points out that institutionalized delinquents may be unrepresentative of the entire range of juvenile delinquents, because of the biases of judges and the police in respect of social class. He notes that institutionalization may well change the child's relation with his parents—thus making it difficult to determine whether a phenomenon such as parental rejection precedes or follows incarceration. He argues, too, that, since judges are more likely to send children to reform school if their parents are separated, it is possible that broken homes may not be causally related to the onset of delinquent behavior.

In a laudable (but ultimately fruitless) effort to establish a more valid criterion of delinquency, Nye constructed a short (and statistically reliable) questionnaire. In it he asked a variety of questions about mild forms of misbehavior. ("Have you ever driven a car without a driver's license?" "Have you ever skipped school without a legitimate excuse?" "Have you ever defied your parents' authority to their face?") The items described acts which, at most, constitute slight deviance ("Have you ever had sex relations with a person of the opposite sex?"). Nye then adminis-

tered his questionnaire to students in the high schools of three small western towns. From their answers he differentiated children who were "most delinquent" from those who kept clear of "delinquent" behavior.

It is surprising to note that Nye—after his criticisms of institutionalization as the criterion of delinquency—validated his questionnaire on a population incarcerated in reform schools. He comments that the validity of the scale is established by "its ability to predict the more serious and frequent delinquent behavior characteristic of institutionalized adolescents." However, 14% of the delinquents in the reform school were tabbed as nondelinquent by the questionnaire. This result may have emerged because certain questions dealing with serious acts—"Have you used or sold narcotic drugs?", "Have you ever taken things of large value?"—had to be eliminated from the final high-school "delinquent behavior" scale. Although the incarcerated delinquents admitted these acts, the high-school students did not. Thus, by using the incarcerated sample as his only major base for validation, Nye is implicitly forced to admit the conclusion already reached by most criminologists: that the community's decision—whether represented in prison commitment, court conviction, or police arrest—is the only way we have of defining that amorphous term *delinquency*.

ALTHOUGH Nye's study cannot be considered an analysis of serious delinquency, his conclusions, nevertheless, have wide interest. His work is the first serious effort to measure the extent of minor deviant acts in a high-school population. He is, moreover, almost unique in making a systematic comparison between the factors leading to misbehavior in girls as well as boys.

Most of Nye's correlations between familial relationships (again measured by a questionnaire administered to the high-school children) and 'delinquency' are familiar to social scientists. 'Delinquency,' he found, is related to such presumed antecedents as unhappiness within the family, mobility, parental rejection of the child (and the child's rejection of his parents), lack of control,

and a factor which might be called "parental sloppiness."

At only one point do Nye's results differ strikingly from those of previous investigators. Nye concluded that delinquency is not more common in the lower class, for his questionnaire revealed no difference between middle-class and lower-class children in their reports of misbehavior. This conclusion is open to three criticisms: first, we cannot assume that the kinds of behavior described in the questionnaire are actually serious delinquencies; secondly, even if we could make this assumption, it would follow that Nye should have discovered *more* delinquency among his middle-class subjects. If he is correct in believing that social bias brings a disproportionate number of lower-class children to reform school, then he should have found a greater proportion of middle-class delinquents who were left "free" in the high-school population. Finally, it is possible that the middle-class high-school students cooperated more fully with the research and thus gave a more candid self-report of their misdemeanors. Otherwise, how are we to reconcile these findings with, say, Kinsey's reports of the more frequent sex experience at this age in the working class?

Since all of Nye's correlations stemmed from (a) the way the child described his own behavior and (b) the way he described his parents, the research is left open to a further inquiry: Are the children's reports subject to a halo effect? Those children who did not care for their parents tended to say that their parents were quarrelsome, that their parents did not love them, that punishment was unfair, strict, inconsistent, irrational, and unexplained, and that their parents often withdrew their love. Since these same children reported a higher frequency of 'delinquent behavior,' what may at bottom be a single correlation may well have proliferated into many.

The virtue of Nye's work lies in his empirical treatment of such problems as male-female differences and of 'delinquency' in an average high-school group, and also his attempt to eliminate certain unfortunate, yet inevitable, biases in the definition of crime.

D. H. Stott, on the other hand, specifically denies any desire to furnish empirical proof for the many generalizations contained in his book. "If mankind," he writes, "had always awaited formal proof before accepting new knowledge we should still be in the Early Stone Age. . . . Ultimately, proof only amounts to convincing other people that what one says must be correct." Social science is irrelevant to Stott's basic goal. He hopes, through his analysis of various family patterns encountered in the practice of social work, to be of practical assistance to the caseworker, counselor, or teacher.

STOTT's work is essentially taxonomic. He has classified a large variety of cases of "unsettled children" (mainly delinquent, but also children otherwise disturbed) into twelve diagnostic categories. While he believes that there is one basic factor underlying all breakdown in children—namely, rejection of the child by the parent, manifested in such a way that "the child feels its membership in a family group in jeopardy"—he distinguishes a number of "types" of family situations that threaten a child's security, and discusses possible treatment. The discussion of treatment is particularly interesting, in that it ranges from detailed matters (such as interviewing methods which a social worker may use to allay defenses and establish communication with malfunctioning parents) to broad issues of social service and penal administration, like commitment and parole policies.

Stott's "exploratory and inductive" explanation of how families affect their children inevitably leaves the social scientist dissatisfied. He makes no attempt, for example, to define crucial words in an operational manner; terms like *natural affections*, *adverse living conditions*, *hostility*, *anxiety*, *normal*, *very variable attitudes* and *unforthcomingness* are used in an ambiguous fashion. Nor does he attempt to offer empirical proof for such sweeping statements about the family as "Where there is no affection, there is no sense of obligation or duty, and no morality." Nevertheless, if Stott's book is scientifically unpalatable, it does indeed offer a number of prac-

tical insights which should be very helpful to the social worker. It is well written, and its case histories give it a "human"—and humane—touch that is absent in statistical studies like Nye's.

Psychoanalysis Still Unanalyzed

Jules H. Masserman (Ed.)

Science and Psychoanalysis. Vol. I: *Integrative Studies*. New York: Grune & Stratton, 1958. Pp. vi + 201. \$5.75.

Reviewed by FRANK AULD, JR.

who is now Associate Professor of Psychology in Wayne State University in Detroit, having recently moved on from Yale University, where a few years ago he collaborated with John Dollard and Alice White on *Steps in Psychotherapy* (Macmillan, 1953). CP gets about one review a year out of him. The last was a composite of three books, respectively by Reik, Rickman, and Bergler (CP, June 1958, 3, 167-169).

A READER of this book who believes in titles is bound to be disappointed, for the volume has very little to contribute to the solution of one of psychology's most urgent problems: "What kind of a science is psychoanalysis, if indeed it is a science at all?" And the psychologist reader will learn also that he is ineligible to become a full member of the organization that sponsored the symposium from which the book is derived, the Academy of Psychoanalysis.

Though smarting from this rejection, the psychologist will still turn hopefully to a book whose title promises so much. He will find, first of all, a competent review by David McK. Rioch of some research on how men and other animals organize their behavior temporally and spatially. Rioch, however, does little to link these researches with the problems and data of psychoanalysis.

Next is a review of *The Pre-Freudian Origins of Psychoanalysis*, written by Walther Riese, a medical historian.

Riese seems to assume: if an idea was published before Freud's time and became a part of philosophic thought, the idea was decisive for Freud's adoption of a similar idea. We need to be reminded, of course, that we are all creatures of our cultural milieu, and that Freud was of his. But Freud learned above all from his patients; it was their behavior that forced new formulations out of him. It is a matter of regret, therefore, that the erudite and judicious Riese has neglected to show the interaction in Freud's creating between prior assumption and current data, to show to which assumptions of prior philosophers and scientists Freud actually held and how these assumptions shaped his work of discovery. This chapter comes close to justifying the book's title, yet disappoints us because the author neglects the vital empirical side of science.

A paper by George Devereux gives his views about how anthropologist and psychoanalyst should consort together. One must score his chapter as concerned with the strategy of scientific advance but lacking in solid facts about past fusions of psychoanalysis and anthropology. Jurgen Ruesch attacks "verbal language" and praises nonverbal communication. Ruesch argues that, if psychiatrists communicated with each other more by nonverbal means and less by words, they would understand each other better. It may be so, but what does it have to do with the book's theme? Edith Weigert is next, with some remarks about problems of communication in psychotherapy, a quite skeletonized presentation. Again: her comments are irrelevant to the book's purported theme, though one can imagine ways in which they could have been made relevant.

THE title of the chapter by James Grier Miller—*Psychoanalysis and Systems Theory*—raises our hopes again. Miller first describes Freud's theories of the mind. This reviewer feels that Miller garbles Freud without actually making any misstatements. One of his sentences is especially puzzling: "Freud made a distinction between a 'primary process' against pain and repression, a process excluding from consciousness what would be unpleasant to the ego."

In *The Interpretation of Dreams* Freud did say, "There will be an inclination in the primitive apparatus to drop the distressing memory-picture immediately, if anything happens to revive it, for the very reason that if its excitation were to overflow into perception it would provoke displeasure." But as the reviewer understands Freud, the "primary process" is a seeking for immediate discharge of tension, not at all an inhibiting force. Why does Miller link it with the repressing system? The reviewer doubts that Miller's explanation of Freudian concepts will be intelligible to anyone not already well versed in them.

Miller, the reviewer thinks, overstates the difference between Freud's theory in his *Project for a Scientific Psychology* (1895) and his theory in *The Interpretation of Dreams* (1899). The later theory was closely allied to the earlier, differing mainly in the omission of a few neurophysiological speculations, not in change of any principal postulates.

This chapter continues with descriptions of several very recent behavior theories and concludes with a comparison of them with Freud's theories. Thus Miller of all the authors comes closest to fulfilling the book's expressed intention. Nevertheless one feels that the theories discussed, except for Freud's, are so far from sense-data that they cannot very well lead us ahead. Miller says that he understands the importance of forging links to the empirical, yet he seems determined not to do any of the blacksmithing himself.

Roy R. Grinker, the last of the main speakers, contributes a thoughtful and interesting paper called *A Philosophical Appraisal of Psychoanalysis*. Score: not much about science, but Grinker can be forgiven because what he says is so original.

In the last pages of the book Abram Kardiner offers a rebuttal to Devereux's paper, producing a classic of wit, learning, and righteous anger; and Jules Masserman writes feelingly of music as an expression of human need, aspiration, and love. These papers are perhaps worth the price of admission—if the reader can get over his disappointment that the title promises so much more than the book delivers.

Statistical Reunion

Ernest A. Haggard

Intraclass Correlation and the Analysis of Variance. New York: Henry Holt (Dryden Press), 1958. Pp. 171. \$2.90.

Reviewed by QUINN McNEMAR

who is Professor of Psychology, Education and Statistics at Stanford University. Except for three brief interludes, he has been at Stanford since his PhD there in 1932, advancing through the various grades to the top. He claims to be always other things to all men, a statistician to psychologists, a psychologist to statisticians. He likes to worry about statistical method, to confound error, and also to honor rigor and righteousness. He is, of course, CP's Consultant for the huge hospital where psychology's relative frequencies are dressed, and the author of *Psychological Statistics* (Wiley, 2nd ed., 1955).

THIRTY years ago R. A. Fisher utilized the intraclass correlation coefficient as a vehicle for introducing his analysis of variance technique to the research worker. It was a natural union: variances for within and between families provided both a measure of family resemblance and a test of significance for the resemblance coefficient. But analysis of variance was destined to become such a lusty fellow that the union was short-lived, except for occasional cohabitation at the original honeymoon site.

It is the purpose of Dr. Haggard, Harvard-trained Professor of Psychology in the College of Medicine of the University of Illinois, to show in this little book that intraclass correlation need not always lag behind when analysis of variance goes gallivanting in various directions. The extensions are to the situations of two-way and three-way analysis of variance wherein the indistinguishability of scores (within classes) no longer holds as was true of the original conception of intraclass correlation.

The proposed applications are to sundry one-way (within-between groups) setups, to the problem of the reliability of tests, to pattern (or profile) studies involving both individuals and groups, and to rank-order data. The properties of the intraclass R are given, and R is compared and contrasted with the more familiar interclass or product moment r . Throughout the book there is a close integration of R with F , with emphasis on the former as a useful descriptive measure.

Unfortunately, the attempted reunion of these two statistical concepts is encumbered by a poorly organized presentation and a lot of unclear writing. The reviewer found the book frustrating, and all too frequently his struggle for understanding led him to the conclusion that ambiguity is confounded with erroneous ideas. Space will not permit a cataloguing of all the questions that now reside in the margins of the review copy of this book.

On page 33 there is a mix-up regarding an error of estimate. On the same page we are told that, for a given within class variance, R will reach a maximum when the class means are equally spaced. Not so; for said condition and a given range of means, R will be maximal when half the means coincide as the 'highest' and the other half coincide as the 'lowest.' At the bottom of page 55 one finds a statement of assumptions of normality for four sets of deviations in two-way analysis of variance with never a hint that all four are required only for the random model—not for the mixed or fixed models, which require three and one of them respectively.

An intriguing mystery of R is created on pages 63–65 where the measure of agreement in topical space allotments by five textbook authors is an R of .46 or an R of .58, depending on whether the data are conceived as involving the random or the mixed model. (The average of the 10 possible product moment r s among the authors is near .46.) The mystery is deepened by the claims (p. 65) that R based on the mixed model, with 10 instead of 5 authors out of a finite number (15) of authors,

would be .75, and that if all 15 authors were included the R would be +1. Haggard extricates himself from the +1 absurdity, but not from the equally nonsensical .75 nor from the fallacious statement that had all 15 authors been used the topic by author interaction would be zero.

In applying R to profile study, Haggard shows clearly his awareness that test scores must be expressed in standardized fashion with a common mean and common standard deviation before any analysis via either R or F is permissible. But in order to equalize the error of measurement variance from test to test he proposes (pp. 103ff.) that scores on each test be divided by the standard error of measurement for that test. This transformation leads to what are called 'stabilized' scores, and it also unwittingly leads to stabilized chaos: the homogeneity of means and of standard deviations is obliterated. Now the lack of equal score variance from test to test, thus produced, automatically introduces an artifactual component into the person by test interaction, and this inflated interaction in turn is involved in the proposed R s. It would be far better to tolerate heterogeneity of error variances, since it is known that such degree of heterogeneity as is apt to occur in the profile situation will not appreciably disrupt the F test.

The K row of the table at the bottom of page 104 should not include the interaction term. Formula 18 (p. 105) wrongly includes IMS (interaction mean square) instead of residual mean square (as deducible from Table 5, p. 60). This correction will erase the inconsistency of Formulas 18 and 20. When groups are formed on the basis of clusters derived from inverted factor analysis they are, in effect, being grouped according to similarity of profiles, and,

therefore, it is not cricket to apply any F test, as suggested on page 121, for profile differences between the groups so selected.

The chapter on test reliability and errors of measurement is very confusing. One can first question the realism of the "proper experimental design" (p. 84). Just how does one proceed to administer j forms of a test, each form k times (i.e., on k different occasions), and hold occasion constant for j administrations? After all, an individual can take only one form at a time. Three sources of measurement error for an individual's score are defined on page 82 and it is said that "it is possible to estimate the components of variance due to each of the three sources of variation mentioned above and to their interactions" (p. 84). There follows an equation which designates the components in terms of e s (e for error) with appropriate (?) subscripts, and then supposedly from this equation a variance equation is written in which the three main sources of variation are individuals, test forms, and replications instead of the previously defined three sources of variation! The four R s, proposed as measures of four kinds of reliability, are based on the random model with no thought as to whether anyone has ever succeeded in drawing test forms at random from that nonexistent population of test forms. On page 96 it is falsely claimed that person-by-item interaction tends to vanish with an increase in the number of items.

And now the reviewer concludes with two unrelated opinions. The first is that the author has not made a clear case for the meaningfulness of all the proposed extensions of intraclass correlation; and the second is that the impressive list of a dozen names, acknowledged in the preface, can easily mislead readers into expecting a sound book.



The experimental psychologist has learned many of his techniques from the physiologist and mathematician. He may not see how a sound scholar can be interested in such a hopelessly complex subject as the 'human nature' of an individual person.



—STANLEY COBB

The Body's Barriers Go Rorschach

Seymour Fisher and Sidney E. Cleveland

Body Image and Personality. Princeton, N. J.: D. Van Nostrand, 1958.
Pp. xi + 420. \$9.25.

Reviewed by SARNOFF A. MEDNICK

Dr. Mednick is Assistant Research Psychologist in the Institute of Personality Assessment and Research in the University of California at Berkeley. Previously for three years he taught Harvard's course in personality assessment and conducted clinical research with the Rorschach test. He has published many articles, especially on learning and stimulus generalization in relation to schizophrenia, brain damage, and anxiety. Northwestern University gave him his PhD.

DRS. FISHER AND CLEVELAND received their doctoral training at the Universities of Chicago and Michigan, respectively. They have since had the bulk of their professional experience in clinical research and practice in state and Veterans Administration hospitals. In the course of a research project, they made clinical note of the great concern about the body that was characteristic of arthritic patients and the way in which it was reflected in their Rorschach responses. From these observations they developed a scoring system based on responses to the Rorschach test. Their entire orientation is cast in terms of body-image theory.

In terms of this orientation, they hold that Rorschach records may be scored along two dimensions, *Barrier* and *Penetration*. It is assumed that those who score high on the Barrier dimension (giving three or more Rorschach responses concerned with special containers, distinctive animal skins, or protective covering) tend to "experience their body boundaries as definite and firm." Those who score high on the Penetration dimension (giving three or more Rorschach responses concerned

with piercing, wearing away, or violation of the boundaries of objects) tend to experience their body boundaries as "indefinite and vague." The authors' research suggests that firm body boundaries grow from exposure to parents who serve as definite models and that such boundaries provide a stable frame of reference for behavior.

The Rorschach measure makes it possible economically to relate the Barrier and Penetration scores to a host of variables simply by rescored Rorschach records obtained in previous experiments by other investigators. Indeed, the authors take full advantage of the diverse research which has been conducted with the Rorschach. More than two-thirds of the studies they report are based on the rescored data originally collected by others. The authors' intent in presenting these studies is the better to describe the characteristics and correlates of the two Rorschach indices. As it turns out these indices seem to be related to a wide range of phenomena, including site of cancer, occupational choice, heart rate, psychosis, child-rearing practices in Kotra Khomat, Udaipur, muscle action potential, and small group interaction. There remain, however, some important questions that should be raised.

THE single most important item in this book is the Barrier index. Just about every experiment is aimed at determining whether those scoring high on the Barrier index are also high scorers on whatever other variable may be under consideration. (The Penetration index did not prove to be related to very much and is only infrequently men-

tioned.) Thus, in one typical case: "the Barrier score tends to be significantly linked with the *n* Achievement score." However, the authors report in another chapter that the Barrier score is significantly and positively related to the number of whole (W) percepts, a standard Rorschach scoring category. Consequently, it is extremely likely that a good many of the Ss who were high on the Barrier score also gave many W responses. Since the number of W responses on the Rorschach has been shown to be significantly and positively related to this same *n* Achievement measure, it seems fairly certain that some part of the observed relationship between the *n* Achievement and Barrier scores is attributable to the covariation of W and the Barrier score. It is also possible that all of the observed relationship is due to this covariation. In fact it is possible that if the Barrier score were dropped from consideration and only W were used, the degree of relationship might even increase.

We face here a serious methodological flaw which is repeated in one form or another in about every experiment reported in this book and apparently remains unrecognized by the authors. As it turns out, the situation is even worse than so far indicated, since there are several other Rorschach variables (F plus %, response total, and shading total) which are reported to be significantly related in one form or another to the Barrier score. Only six of the Rorschach scoring categories are investigated; four of these are significantly related to the Barrier score. (No analysis is reported of the relationship of the Barrier score to any Rorschach content category or most location categories.) Thus the Barrier score becomes the focal point of the undertaking. Nevertheless they make no attempt to take account of these important covariants. In the entire book I could find no experiment in which the data on these covariants are even reported. Certainly the interpretative significance of these reported findings remains in considerable doubt until these covariant data are provided.

Depending on how these covariant relationships organize themselves we may have either a promising, entirely new

approach to body-image theory which boasts a simple criterion measure, or some relatively unexciting research supporting a variety of old hypotheses. In any case this book, not having initially provided answers to these important methodological questions, is seen to be severely limited in its value.

DESPITE the authors' description of their studies as exploratory, I confess to being disappointed by the tentative, incomplete, and hurried manner in which the experiments are evaluated and reported. There are 27 tables which present results of chi-square tests. In almost half of these differences, the tables show only probability levels without presenting the data or stating the corresponding chi-square values. Unfortunately, in more than half of the tables in which chi-square values are presented, there is *at least one error* in the list of corresponding probability levels cited. These errors all slightly exaggerate the level of statistical significance of the data. They are small errors, but they occur in more than one-half of the tables presented and several times the error carries the chi-square to the .05 level of significance. In addition, despite the fact that all their chi-squares are at one degree of freedom, the authors take the liberal option of not correcting for continuity except where expected cell frequencies drop below 5. They feel that this liberality is acceptable since "the prime goals of the present studies are exploratory rather than the determination of exact relationship." They are, of course, not incorrect in making this choice, but they must face the consequence of reduced reproducibility of results, a limitation which becomes especially serious since it seems likely that the reliability of the Barrier score is quite limited. If in a Rorschach record of 20 responses an individual gave the responses *alligator* and *bobcat*, he would be accorded a Barrier score of two and placed with the Low Barrier group. If, in addition, he had responded with *turtle*, his score would become three and he would now be placed in the High Barrier group. If we were given more information regarding the distribution of the Barrier score, we would be aided in making a judgment

regarding its reliability. However, frequently no data are given. On occasion median scores are presented. Only once are we informed regarding the range of Barrier scores.

In short, the authors have overextended themselves. They give so many different analyses of data in the section devoted to research reports (two-thirds of the text) that there is an average allotment of only 3.8 pages per ex-

periment. It would have been better to restrict the account to a more complete presentation of fewer studies, ones which were better controlled and more clearly relevant. Such studies are to be found among those originated and conducted by the authors themselves or by their students, principally the ones that include the work on the psychosomatic and psychophysiological correlates of the Barrier and Penetration scores.

Und-verbindete Lesungen

Eleanor E. Maccoby, Theodore M. Newcomb, and Eugene L. Hartley (Eds.)

Readings in Social Psychology. (3rd ed.) New York: Henry Holt, 1958. Pp. xi + 674. \$6.75.

Reviewed by ROBERT R. BLAKE

Dr. Blake is Professor of Psychology at the University of Texas, where he was given his PhD a dozen years ago. He has been around a little though: at the Tavistock Clinic in England, in the Department of Social Relations at Harvard, to Mexico, Cuba, Venezuela, as well as to Bethel, Maine, in the interests of his chief love, social psychology. His first research was on stereotypes. Now he is busy with problems of conformity, deviation, and intergroup relations.

THE American way is to measure progress by style changes. Nothing is immune, not even Social Psychology, the discipline responsible for understanding such things. If you shift with the times, you can be modern, keep up with the Joneses, and things like that. If you don't, though, you run the risk of being outdated, stuffy, and old, and who wants that? Women's clothes and automobiles shift once a year. Through three successive versions, the Society for the Psychological Study of Social Issues has set the precedent of a change in *Readings* every five years.

There are advantages to be gained from adopting the '57 model. You can promise your bookstore almost certainly the model will not go out of date till

'62. They can stock up. You can be chic in your students' eyes. The '57 cover is just dazzling: black with white print and with red and white communication-net designs jumping at you from the front cover. Then too, about 55 percent of the interior fittings are different from the '52 (Swanson, Newcomb, & Hartley, 1952), quite sufficient to make a new edition.

Really, though, is it a good deal, this idea of a new *Readings* every five years or so? Do you get something more or better in the '57 than in the '52 or '47 (Newcomb & Hartley, 1947), or should you change over to another kind of *Readings* book? Maybe you should get a Cartwright-Zander *Group Dynamics* (1953), a Hare-Borgatta-Bales *Small Groups* (1955), or a Bendix-Lipset, *Class, Status and Power* (1953). If you are of the conservative kind, perhaps you can afford to be more pedestrian. Ask your students to walk to the stacks and to examine the originals. These are hard decisions to make, whether you are in a group, just together, or simply alone. Furthermore, your decision can make of lot of difference to students. To explore the implications of these statements, it is necessary to know what the '57 model is like.

First and foremost, *Readings* remains

an omnibus of social psychology. Included are sections dealing with *Language and Stereotypes*, *Perception* (social, with motivation and memory thrown in), a *Readings* extra called *Perception of Persons*, *Communication and Opinion Change*, *Interpersonal Influence*, *Reference Groups*, *Behavior under Situational Stress*, *The Socialization of the Child*, *Social Stratification*, *Roles*, including conflict between and among them, *Leadership*, *Group Structure and Process*, and *Intergroup Tension and Prejudice*. It all ends with an *Index*. That's a lot of coverage. To encompass so much you have to keep thin at every point. The number of individual papers per section ranges from three to seven. Five is the average. Correctly the editors make no claim to completeness.

Why were the included areas selected in the first place? Since the reasons are not given, I tried to answer the question for myself. The best single answer is *Tradition*. Most of the sectional titles introduced in the '47 model were continued in '52 and into '57 with relatively little change. These bottles are old, but the wine is new, some of it. There are some changes in sectional titles, though the alterations are mostly in style and not in conception. There has, in the three editions, been continuous reduction in number of sections from 19 to 15 to 13.

Do you get here enough of any single thing to build a solid foundation of understanding or even to generate a modicum of erudition? One way to answer this question is to compare *Readings* with the products of other *Readings* builders. Contrast *Readings*' five papers in *Social Stratification* with the Bendix-Lipset book, subtitled a *Reader in Social Stratification*, with its 60 papers on the same topic. *Readings* has 17 articles under *Interpersonal Influence*, *Leadership*, and *Group Structure and Process*, whereas Hare-Borgatta-Bales include a total of 57 to survey the same area of life, and Cartwright-Zander use 35 to take in the same topics. In the Tagiuri-Petrullo *Person Perception and Interpersonal Behavior* (1958), 23 papers are presented to encompass the burgeon-

ing field of interest dealing with how people look and feel to one another, but *Readings* has only three. By the criterion of efficiency, then, *Readings* can support the claim to cover 'more with less' than any other major source book. A proper conclusion is that if you want a cafeteria or smorgasbord-type of approach to social psychology use *Readings*. If you want a heavier dose of one topical area, you should be better off with something else.

It is customary, and perhaps right, too, to think that things make sense when they fit together to form a whole. Koffka said so, and Lewin said so, and Helson, and Sherif, and others too. Things make sense in a framework. Anything from a thread to a theme to a thesis to a theory should do. When so few articles are presented in any area, as in *Readings*, an editorial orientation, spelling out the importance of each problem area, is clearly indicated. What about *Readings*? Things are presented there without any over-all framework and without specific orientations for the sections. There are no introductions, no editorial comments. Strictly theoretical articles have been avoided. All this is said to be good, because, though teachers vary greatly in theoretical approach, they need reports of sound empirical research to offer their students as a foundation for theoretical thinking. True; but why should this kind of material be deleted from the *Readings*? A teacher might even be aided in improving his own framework if he but had the chance to see how specialists fit things together, to say nothing of the students who need benefitting too. Introductions to the sections are desperately needed.

Would the selection and arrangement of *Readings* have been enhanced if orientational contexts had been provided? I think so. If the editors had written down their frameworks, they might not have produced the book as it appears. Here are some examples: Verplanck's article on the control of the content of conversation might have gone, not in *Language and Stereotypes* where it is, but in *Interpersonal Influence* where it seems better to belong. The Katz and

Brady study of stereotypes, which appears under *Language and Stereotypes* to exemplify the categorization of behavior, might have been placed more appropriately under *Intergroup Tensions* as an example of ingroup-outgroup reactions. The section dealing with interpersonal influence probably would not have run the gamut from Asch's work on conformity through three studies of group decisions, to the Lippitt-Polansky-Redl-and-Rosen study of power. Speaking for myself, I would have been much aided in identifying the thread that connects them, in something other than a spatial arrangement, had the editors helped me in appreciating the common membership character of such a collection. Other makers of *Readers* in social psychology all have provided orienting frameworks for their readers. Perhaps the '62 will fall in line.

Most of the individual papers are good staunch hardheaded social psychology, when each is considered in its own right. The new papers are either of recent vintage or are ones invited by the editors for inclusion in this edition. One flavor you got from the '47 model was that you were reading 'classics.' Now you get the idea you're reading 'moderns,' and that, here and there perhaps, sociometric considerations prevailed in their selection.

Readings is good reading for what it is: an eclectic sample of research from social psychology. The assembly job, however, leaves something to be desired: the blueprint.

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Private Practice in Clinical Psychology

By *Theodore H. Blau*. This new book is recommended for psychologists who wish to practice in a community setting. It is the first book in the field which deals specifically with the usual and unusual situations and problems faced by the psychologist in private practice. It provides a detailed discussion of the interest in private practice to date and a careful review of the background and experience necessary for practice. The types of people who are likely to apply for help are discussed. Methodology, such as the taking of case histories is fully treated. A discussion of psychotherapy is included. *To be published this Fall.*

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Does Psychoanalysis Reject Hypnosis?

Milton V. Kline

Freud and Hypnosis: The Interaction of Psychodynamics and Hypnosis.

New York: Julian Press, and Institute for Research in Hypnosis Publication Society, 1958. Pp. xii + 207. \$4.00.

Reviewed by FRED J. GOLDSTEIN

Dr. Goldstein is Chief of the Department of Clinical Psychology in the Los Angeles Psychiatric Service, and he also teaches internes, and history of psychology at the University of Southern California. He began his professional life in the east with Gardner Murphy but presently turned up at the University of California at Berkeley, where Egon Brunswik refereed his internal struggle between positivism and psychoanalysis, while Edward Tolman, Else Brunswik, and Leo Postman coached from the side lines. He came out a toughminded analytical clinician with an intense interest in Freud, which he still pursues.

THE author of this book has an eminent position in the field of experimental and clinical hypnosis. He is not only a Lecturer in medical hypnosis and a Research Project Director (in hypnosis) at Long Island University, but is also Editor of the *Journal of Clinical and Experimental Hypnosis*, has written several books, and more than forty articles in this field. He is a member of the American Psychological Association, the Academy of Psychosomatic Medicine, American Academy of Psychotherapy, and is, of course, a prominent member in the Society for Experimental and Clinical Hypnosis. It seems quite understandable that such a dedicated individual would take up the cudgel in defense of hypnosis and attack the indifference of Freud as he does in this book.

Kline does indeed attack Freud's views of hypnosis, presenting experi-

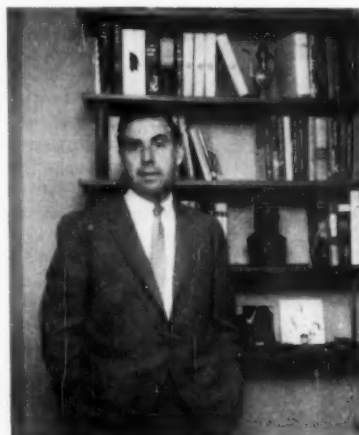
mental and theoretical sections on work done by himself and bearing on the status of hypnosis today. In critically evaluating his arguments against Freud, it seems particularly apt to quote Freud's response in 1889 to Meynert's vilification for his support of hypnosis: "The respect for greatness, especially intellectual greatness, belongs to the best qualities of human nature. But it should take second place to the respect for facts. One need not be ashamed to admit it when one sets aside reliance on authority in favor of one's own judgment gained by study of the facts."

In light of this quotation, let us consider the main argument advanced by Kline that Freud rejected hypnosis, and, further, that, because of Freud's authority, psychoanalysts have maintained the master's opinion intact sans knowledge of the facts of this area. Kline's arguments derive from *The Studies on Hysteria* (1895) and *Group Psychology and the Analysis of the Ego* (1921), where Freud's early experiences with hypnosis and his later ideas about them have been set down. The quoted excerpts demonstrate to Kline how authoritarian and unskilled Freud was as a hypnotist and also how unaware he was of the manifestations of transference and resistance in hypnosis. With an eye on the initial quote above, let us study the facts.

Freud did not just take up hypnosis one week and reject it the next. He was actively supportive of hypnosis in the 1880s and translated four books on the subject, two by Charcot (1886, 1892-94), and two by Bernheim (1888, 1891-

92). He also wrote several favorable reviews on hypnosis for the *Wiener Medizinische Wochenschrift*, dating from 1886 through 1890. In 1886 Freud gave the report of his trip to Charcot before the Vienna Medical Society, occasioning considerable difficulty for himself by espousing the unpopular views of Charcot about hypnosis and hysteria. Later Freud's initial criticism of Bernheim was followed by his visit to Nancy in the summer of 1889 for several weeks to learn from Liébeault and Bernheim and to perfect his hypnotic technique. The personal and professional difficulties that followed Freud's espousal of hypnosis and Charcot have been well documented by Ernest Jones and in Freud's own writings. Meynert's bitter fight against Freud and hypnosis are also part of Freud's pro-hypnotic period.

Is Kline wrong then in selecting Freud's own writings to support his position? This reviewer believes that there is here a misplaced emphasis, produced by taking quotes out of a larger context, one that distorts the total picture. Freud wrote so voluminously, changing his ideas as he wrote, that any isolated criticism from one minute part of his writings is rarely as critical or appropriate as at first it appears to be. In this book, Kline supports a position regarding hypnosis, one which is intended to recruit others to work in his own hypnotic vineyard that they may reap the promised yield of facts and understanding which he thinks they will find.



MILTON V. KLINE

He has every right to this position, as also has Freud to choose other paths toward knowing people.

FREUD's initial focus was on learning how to treat patients that were not helped by the known treatments of his day. It is patent that Freud was authoritarian in his hypnotic technique, unaware of the transference, and most of the time insufficiently aware of the manifestations of his patients' resistance in hypnosis. Not to have conceptualized resistance from hypnosis is not culpable, for no one else noticed it either. At times, Freud was aware of dynamic phenomena but only vaguely, as a careful reading of the four cases treated by Freud in *Studies in Hysteria* would show. Certainly such facts as patients' indicating that they were not asleep despite his pronouncements that they were, and that he could not hypnotize a good many, helped to steer Freud from a continued reliance on hypnosis diagnostically and therapeutically. Freud's concentration technique and the current ritualism of the couch, free association, and unconscious motivation undoubtedly stem from his experiences in his initial use of hypnosis with patients. One might, however, with equal justification, criticize Freud for blindness to transference in massage treatments, for he used massages over the "whole of the bodies" of hysterics, electric stimulation of anesthetized thighs, baths, and similar forms of the usual treatment without noting the erotic nature of the transference involved. Also, Freud invited patients to dine at his home, again suggesting his extreme naïveté regarding transference as judged by current standards. Freud, of course, did not have Freud to build on, and such treatments were in vogue in his time. If one wanted to, he could readily build up a case for sexual traumata, as fantasied by his hysterical patients, being a symbolic representation of the erotized (hypnotic and massage) transference occasioned by his use of varied physical, directive, hypnotically assaultive, and probing techniques. (There is probably some truth in this possibility, as certainly there is in what Kline points out as Freud's resistance to learning "the facts" regarding hypnosis.)

As to Kline's suggestion that hypnosis is being relegated to the dust heap by psychoanalysis, that is also a debatable point. It is true that Freud at 66 (1921-22) seemed to dismiss this technique from further therapeutic consideration. Ferenczi's early theorizing was accepted by Freud to explain the hypnotic phenomenon, as Kline points out. A number of psychoanalytically trained people have tried to explore the hypnotic technique with a psychoanalytic frame of reference. Schilder was one of the most original of thinkers about this phenomenon, and, like Kubie after him, viewed it from both a physiological and psychological perspective. Others from the field of psychoanalysis who have either theorized about hypnosis or experimented with it, include such people as Hartman, Grinker and Spiegel, Brenman and Gill, Fischer, Nachmansohn, Lindner, and Rado. Recently, the name hypnosis has appeared more frequently with people like Wolberg, attempting to combine psychoanalytic and hypnotic techniques for psychotherapy. If hypnosis has been neglected as an area for serious research, it seems to have survived such neglect rather well, and to have suffered only the same fate as all other new additions to the psychotherapeutic family. Psychoanalysis, certainly in America, has unfortunately become more of a treatment specialty than a research tool; but, fortunately, steps to modify this trend are planned and expected.

In conclusion, I would say that the critical comments of this book are intriguing, and the book well worth reading for its focus on Freud's avoidant attitude towards hypnosis, as well as for background in creative theorizing and experimentation with hypnosis. Freud, in his own writings, seemed open enough publicly to expose his mistakes and obstinate enough to maintain his earlier position until forced to change by the observed facts or by his own subsequent appraisals. He had to see such things for himself before any genuine acceptance was forthcoming. Yet, if I read the meaning of the hypnotic literature correctly, clear evidence of the nature of hypnosis is yet to come. People as devoted and creative as Kline may ultimately correct this state of affairs,

and, when this happens, let us hope that such contributions will be evaluated in the context of the period in which the author worked.

A Sound, General Text on Learning

James Deese

The Psychology of Learning. (2nd ed.) New York: McGraw-Hill, 1958. Pp. x + 367. \$6.50.

Reviewed by DONALD J. LEWIS

who is an experimental psychologist who has lived with rats for a decade and knows a very great deal about human and rodent learning and theories of learning. He has a doctorate from the University of Southern California, was at Northwestern University for seven years, and is now Associate Professor of Psychology at Louisiana State University.

THERE are very few undergraduate textbooks on learning, but one of these few is now in its second edition. Because of the paucity of similar texts, an adequate perspective is difficult to obtain, but at least it can be said that Deese's *Psychology of Learning* is the best in the field and is a pretty good book by almost any standard. The first edition was published in 1952 and was a good book. The second, however, is superior. It is more smoothly written and more comprehensively organized. The first book at times seemed to borrow its organization, by sections, from other sources. The section on conditions of reinforcement, for example, followed very closely the organization presented by Hull in his *Principles of Behavior*. For the second edition, Deese has had more time to organize his own book, and the result is quite pleasing.

James Deese received his PhD a little over a decade ago from Indiana University. From there he went to Johns Hopkins University where he has been ever since. His research has been quite varied. His publications have been in

most of the areas of experimental psychology, ranging from the physiological to the definitely social. His present interest seems to be mainly in language and psycholinguistics, and his book shows the result of this interest in that it contains several interesting passages on these specialities.

Deese considered his first book to be a survey of the field of learning, but he makes no such claim for the second. Although he treats fewer topics, very little of important contemporary or historical interest has been left out. There is material on classical conditioning, verbal learning, problem solving, imprinting, mathematical models, and physiology. A large variety of theoretical points of view are fairly and critically considered. Only those who think their particular learning hobby is all or a major part of learning will be greatly disappointed by the coverage.

Deese is not overly dogmatic or doctrinaire in his treatment of the various theoretical points of view, and probably only the dogmatic or doctrinaire will find serious fault with his criticisms. He may be a little harsh on statistical learning theories, and his comment that Spence's recent book, *Behavior Theory and Conditioning*, probably stands as the final and culminating work of the Hullian school will undoubtedly raise a few smiles. He is greatly impressed with information theory, the autocorrelation techniques, and the analysis of behavior sequences. But surely, the writer of a textbook may be permitted to express a few opinions.

ALTHOUGH its virtues are not inconsiderable, one can still find a few features that might have been different. In citing animal experiments, Deese is much too prone to have his rats predict events. He provides them with wills and allows them to choose their behavior. They are altogether too clever for an experimental laboratory. He makes no mention of some of the major changes in Spence's thinking, e.g., his new position on delay of reinforcement. On several occasions, Deese belittles the "simple stimulus and response paradigm" without being clear that the basis of all purely psychological observa-

tions is stimulus and response. He maintains, rather idiosyncratically, that the avoidance of pain is a need that exists at a more or less constant level, awaiting the occurrence of an appropriate stimulus to set it off. One could mention other aberrations, but even in sum they are overshadowed by the book's virtues.

Perhaps the only serious adverse criticism of this volume is that it seems at times quite remote from the laboratory. Much of the discussion is at the level of experimental conclusions rather than experimental data. This reviewer would prefer to have had more descriptions of actual experiments. There are some such descriptions, to be sure, and there are close to sixty graphs and figures of data and apparatus. And it is undoubtedly true that to present more data and experimental descriptions, something else would probably have had to be omitted.

Perhaps it is possible to get an idea of the content of a book by examining the list of bibliographic entries. Six psychologists have ten or more entries. These, with the number of entries, are: Hull, 14; Spence, 12; Deese, Hovland, Thorndike, and Underwood, 10 each. Skinner has 3 entries. If one looks at the number of different pages on which these psychologists are mentioned—as determined from the index—Skinner is the only one whose position changes drastically. Mentioned on the largest number of pages is Hull at 37, then Skinner at 25, Deese and Underwood at 21, Thorndike at 19, Spence at 15, and Hovland at 14. It is not clear what conclusions should be drawn from this counting exercise; data are sometimes engaging even without discussion.

Deese has presented a balanced, sound, and well-written book. It should find wide acceptance in departments of psychology and in those departments of education which maintain an interest in learning.



All life is an experiment. Every year if not every day we have to wager our salvation upon some prophecy based upon imperfect knowledge.

—OLIVER WENDELL HOLMES

The Fateful First Year

D. W. Winnicott

Collected Papers: Through Paediatrics to Psycho-Analysis. New York: Basic Books, 1958. Pp. x + 350. \$6.50.

Reviewed by ROY SCHAFER

who is Associate Professor of Psychiatry in Yale University's School of Medicine and Chief of the Clinical Psychology Service there. He has worked with quite a number of distinguished psychiatrists and psychologists, but the most important is David Rapaport, first at the Menninger Clinic and later for six years at the Austen Riggs Center. He is the author of three books on testing, the latest being *Psychoanalytic Interpretation in Rorschach Testing* (1954).

FOR over twenty-five years (1931–1956), D. W. Winnicott, distinguished British pediatrician and psychoanalyst, has been creatively studying the conditions that promote and disrupt the healthy development of the infantile "psyche-soma." *Infantile* here means the first year of life, the first months, the first days, and, in one bold paper, even prenativity and birth. Faithful to his original identity as pediatrician, Winnicott remains concerned with the real world's adaptation to the infant's body and mind and to its effects on the infant's and later the adult's adaptation to the real world.

Combining the psychoanalytic functions of observer, therapist, and theorist, Winnicott calls on his extensive experience with the infant in his mother's lap during the medical consultation, with the child in analysis or under psychoanalytic management, with the antisocial personality, and with the adult depressive and schizophrenic in analysis. He by-passes ordinary neurotics, believing them to have been adequately accounted for by "ordinary psychoanalysis," that is to say, Freud's understanding of the vicissitudes of the oedipus complex. (Of this implied

completeness of understanding we are bound to be dubious. Winnicott's contributions alone suggest modifications of id-ego-superego theory, and hence, of the concept of neurosis.)

The ordinary neurotic attains interpersonal relationships, disturbed though these may be. In contrast, the subjects of Winnicott's research live fundamentally outside the world of interpersonal relationships. Although they appear to live in social reality, they do so only by means of a "false self"—a false self that develops to hide and protect the rudimentary true self, after the development of the latter has been blocked and fragmented by traumatic failures of mothering at the beginning of life. These patients have not even matured to the infantile "depressive position" described by Melanie Klein or what Winnicott now prefers to term the "stage of concern" with the possible destructive effects of one's aggressive loving of the mother. (While Winnicott is avowedly much influenced by Klein, he is also avowedly intent on formulating his own—and his patients'—version of Klein as well as of Freud. Such an intent is altogether consistent with the spirit of psychoanalysis even though it makes more difficult a synthesis of psychoanalytic theories.)

These patients live in an internal world of persecution by images and body experiences, a world of attempted magical control as a substitute for the control not provided by the environment at the critical time. Their therapy must unfold this strange, incredibly powerful, inner reality. By "management," that is to say, by meeting these infantile needs in appropriate maternal modes (akin to Sechehaye's "symbolic realization"), the analyst enables the true self to come out of hiding and a true ego to resume development. The patient may then grow into useful instinctual experience and real interpersonal relationships. Only afterwards may he be analyzed by 'ordinary analysis.'

IN these few paragraphs I have given only a small sample of the scope and complexity of Winnicott's hypotheses and observations, and none of his technical elasticity, boldness, and inven-

tiveness. Of his pediatric-psychoanalytic technique, perhaps the two outstanding characteristics are *restraint* and his inner *confidence* that the patient, unmolested by interpretative zeal and allowed to regress (as indeed he must) within the context of a protective, undemanding, understanding therapeutic situation, will define and communicate his magical world and so discover and rescue his true self. One does wonder, however, at Winnicott's relative neglect of the often observed *continuing* reinforcement of infantile traumata by a disturbed family environment, and *continuing* harassment that prevents a true self from getting at least partly organized during the pre-oedipal years and the latency period. Our author's penetrating analyses of adequate mothering are confined, in effect, to the first year of life, and fathering is virtually ignored by him.

Over the years, Winnicott's conceptualizations have increasingly taken on a personal flavor. His language tends toward the same near-poetic mode, the sensibility-in-depth used by other major Freudian investigators of the origins of the self and the ego, like Erik Erikson. (Like Erikson, Winnicott also stresses the healthful potentialities of developmental crises.) Perhaps any attempt to put into words an experience that pre-dates words, whole and steady objects, and a defined and continuous self, must inevitably be metaphoric, allusive, a record of empathic experience, and at times even an adultomorphic demonology. Unlike poetry, however, these formulations are tied to intensive, self-disciplined, partly standardized observations of people in treatment, and they are steadily informed by basic psychoanalytic theory. Also, these empathic conceptualizations are or can be interrelated with the growing body of more traditionally conceptualized studies of the ego, the self, and adaptation, studies contributed by Anna Freud, Hartmann, Rapaport, Jacobson, and others.

Winnicott's contribution will be best appreciated by those with psychoanalytic training and experience, particularly those among them who have treated psychosis in adults or severe disturbances in young children.

How to Use Social Psychology

F. E. Emery and O. A. Oeser, with the assistance of Joan Tully

Information, Decision and Action: A Study of the Psychological Determinants of Changes in Farming Techniques. Melbourne: Melbourne University Press; New York: Cambridge University Press, 1958. Pp. xiii + 132. \$3.75.

Reviewed by KURT W. BACK

who is Associate Professor of Sociology at Duke University and also Associate Professor of Medical Sociology in the Department of Psychiatry. Earlier he worked with the Research Center for Group Dynamics at the Massachusetts Institute of Technology and then at the University of Michigan. In CP he has reviewed Hofstätter's Gruppensdynamik (Oct. 1957, 2, 260).

A PSYCHOLOGIST, faced with a demand to solve a concrete social problem, must ask himself a painful question: Can the general concepts of social psychology be applied directly or does the problem have to be studied by itself? Most of the hypotheses or relationships of social psychology are so tentative and are derived under such special conditions that it is hazardous to adapt them as rules of conduct in actual situations where many uncontrolled variables reign. The safest way open is thus to start with a detailed study of each practical problem, finding hypotheses as one goes along and trying eventually to fit the results into the main body of science. This procedure is clearly not the most satisfactory one for the development of theory.

The authors of the present volume, F. E. Emery and O. A. Oeser, were faced with a problem of this kind. On the faculty of the University of Melbourne, they had been asked by the Commonwealth Scientific and Industrial Research Organization why the rate of adoption of the results of technical agricultural research has been so slow.

Their attack on the problem consisted of an almost direct application of a theory of the conditions of successful communication, a theory which has been developed by Heider, Newcomb, Cartwright, and Harary. The main feature of this theory is the concept of 'balance' or 'strain toward symmetry,' that is to say, the effect of a communication will tend toward the condition in which the attitude of the recipient toward the source and his attitude toward the communication itself are identical. Thus the investigators' task was to find the conditions under which the position of the farmer toward the sources of technical information is favorable.

The present study is designed primarily to show that this approach is possible and that it can be fruitful. It was limited to a sample of 36 families who were subjected to searching interviews. From the resulting data the authors constructed five indices of conditions predisposing to change: scale of farming, urbanizing influences, conceptual skill, situational motivation, and exposure to mass media. In addition, several criterion measures for adoption of modern farming techniques were derived. In general, the five indices turned out to have highly significant relationships with adoption of new farming techniques. The practical importance of the study was thus demonstrated: agricultural officers are seen to have, as a result of the study, the means of determining who would be most likely to adopt new practices. It also becomes clear on what topics they should concentrate their work.

So much for the value to the practitioner. How far has the theory been confirmed or modified? It can be seen that three of the measures—scale, urbanization, and conceptual skill—reflect relatively permanent states of the recipient; the fourth indicates motivation, and the fifth the possibility of being influenced. Since there is no direct measure of the attitude toward the source of the communication, a rating of the agricultural officer, any test of a theory of perceptual balance or strain toward symmetry is precluded.

The research thus turns out to be addressed to two kinds of audiences. It shows to the practitioner that social psychological investigation can have value in designing a strategic approach to change. It shows to the psychologist that the use of theory in applied research is possible. The ingenious way in which indices of conceptual variables were constructed from a few simple questions is remarkable and may well be the greatest scientific contribution of the volume.

The limitations of the study are equally clear. It keeps middle ground between a survey of a population and a

collection of case studies. The authors end with a typology of 32 classes (five variables dichotomized). Their small sample size prevents them from large generalizations and they refuse to consider individual dynamics. "But that would be literature," they say, "not psychology. The District Agricultural Officer and other extension workers, like applied psychologists, need to know the individuals; the research psychologist, as a scientist, must rest content with the typology." Within these restrictions, which they accepted consciously in their research design, the authors have prepared a model of careful research and have made excellent presentation of it.

What it is to be Well

Marie Jahoda

Current Concepts of Positive Mental Health. (Monograph Series, Joint Commission on Mental Illness and Health, No. 1.) New York: Basic Books, 1958. Pp. xxi + 136. \$2.75.

Reviewed by OTTO KLINEBERG

Dr. Klineberg is Professor of Psychology at Columbia University and Co-chairman of Graduate Studies in Social Psychology. For over a quarter of a century he has devoted himself to the study of tensions and the promotion of understanding between nations and races. He has between 1935 and the present time pursued these ends on various appointments in China, Germany, Brazil, and France, as well as in the USA including Hawaii. Recently he was the Head of UNESCO's Division of Applied Social Science. He has written eight books, from his Race Differences in 1935 to his Race and Psychology in 1951. Columbia University awarded him the Butler Medal in 1950 for his Tensions Affecting International Understanding. (It is the medal that R. S. Woodworth was awarded in 1918 for his Dynamic Psychology. Woodworth uses it for a paper weight.) Then in 1956 Klineberg was given the Kurt Lewin Memorial Award by the Society for the Psychological Study of Social Issues.

THIS valuable little book by a distinguished psychologist, formerly professor at New York University, now Senior Lecturer at Brunel College of Technology in London, England, represents a significant contribution to our understanding of the meaning of mental health. For a number of years the revolt has been spreading against the purely negative conception of mental health as the absence of mental illness, although this conception apparently still prevails among many clinicians. It is one thing, however, to affirm one's concern with *positive* mental health, and quite another to present a satisfactory indication of just what is meant. It is to this problem that Dr. Jahoda addresses herself.

The problem was recognized in the position taken by the World Health Organization at the time it was founded: health, including mental health, was to be regarded not just as the absence of disease, but as a positive state of "well-being." Although this view undoubtedly represented an important step forward,

Dr. Jahoda rightly feels that the term *well-being* is much too complex and indefinite for purposes of definition, and that it requires much more clarification and specification if it is to serve as a useful guide for either action or research. Her own critical survey of the literature indicates, however, that there have been so many different conceptions of positive mental health, that it is not too surprising that the WHO did not attempt to be more precise in presenting its philosophy and its program to the international community.

JAHODA distinguishes six major approaches, all of which contribute something to what turns out to be a necessarily composite definition. First, there are the attitudes of the individual toward his own self, his degree of self-acceptance of both his limitations and his potentialities. Related to self-acceptance is the correctness of his self concept, the accessibility of his 'self' to consciousness. In all of this, the sense of identity is important, the knowledge by the individual of what and who he is.

A second approach sees mental health in terms of growth, development, self-actualization, maturation, and the unfolding of one's potentialities. Third, there is the criterion of integration, self-consistency, the presence of a unifying philosophy of life (which may not necessarily be clearly or consciously formulated). A fourth criterion is the degree of autonomy or independence; a fifth is adequate perception of reality and freedom from need-distortion; the sixth and last calls for environmental mastery, adaptation and adjustment, adequacy in love, work, and play, and in interpersonal relations in general.

Jahoda does not choose between these various approaches, although she has some pertinent critical comments to make regarding each one. She holds that, since no one of the definitions is completely satisfactory, we should accept the conclusion, first, that there may be varieties of mental health, and, second, that a multiple criterion is needed. "At the present state of our knowledge it may well be best to combine the idea of various types of health with the use of a multiple criterion for



—Conway Studios

MARIE JAHODA

each. The former will prevent overgeneralizations; the latter will permit us to do justice to the complexity of human functioning" (p. 73). One might, of course, raise the question, however, as to whether all of the six criteria, or all their subdivisions, should be equally weighted in such a multiple criterion.

This reviewer has the impression that Jahoda is herself partial to the fourth approach, the one relating to autonomy or independence. In dealing with the problem of values which enter into our understanding of mental health, she writes: "By way of example, one value strikes us as being compatible with almost all of the mental health concepts discussed here: an individual should be able to stand on his own two feet without making undue demands or impositions on others" (p. 80). She then goes on to raise a very important issue: "whether it is meaningful outside the orbit of Western civilization is a moot question" (p. 80). This question is indeed of great interest, and an intensive cross-cultural investigation will be needed before we can determine to what extent our Western concepts of mental health are exportable to other parts of the world. The World Federation for Mental Health is now engaged in preliminary exploration along these lines.

Jahoda ends the main body of her discussion with suggestions for research on the various criteria which have previously been reviewed, as well as on the environmental conditions related to the acquisition and maintenance of mental health. This is a rich section, which cannot easily be summarized. Many of the research ideas struck this reviewer as ingenious and potentially rewarding, but he found the presentation in tabular form (pp. 96-99) more confusing than helpful. The question arises as to who reads "suggestions for research," and there is a difference of opinion regarding their usefulness. This particular list, however, contains a number of interesting ideas that deserve attention.

In a final chapter written by Dr. Walter E. Barton on the *Viewpoint of a Clinician*, the opinion is expressed that the first task is to reduce mental illness, not to concern ourselves about positive mental health in Jahoda's sense. "If we had solved, or even partially solved, the problems of preventing or treating major or minor mental illness, we could then justifiably concern ourselves with the issue of superlative mental health, or the degrees of goodness in good mental health" (p. 111). Further, "I believe most patients would settle for the absence of illness. If they are not sick, they are well" (p. 119). Of course Dr. Barton is right that everything possible should be done to cure the sick, but the positive approach so ably developed by Dr. Jahoda raises our sights to a more important, if even less accessible, goal.

She herself ends on this note. "If policy makers open the way to the acquisition of further knowledge, if practitioners in the mental health field co-operate with scientists in thoughtful experimentation, if the fruits of research can be applied without losing respect for the infinite diversity of human beings, concern with mental health may improve the quality of living" (p. 110). This aspiration is beautifully expressed, but would it be unfair to ask her to write another book, soon, on *Current Concepts of the Quality of Living*? Whether or not she accepts this friendly challenge, we are in her debt for what she has already given us.

Momentous Moments

George Schwartz and Philip W. Bishop (Eds.)

Moments of Discovery. Vol. I: *The Origins of Science*. Vol. II: *The Development of Modern Science*. (Foreword by Linus Pauling.) New York: Basic Books, 1958. Pp. xviii + 500; xii + 501-1005 [505]. \$15.00 the set.

Reviewed by EDWIN G. BORING

Dr. Boring is Edgar Pierce Professor of Psychology Emeritus at Harvard University, the author of *A History of Experimental Psychology* (Appleton-Century-Crofts, 2nd ed., 1950), a Freshman geront, a 4-5-2, in both physique and temperament (according to W. H. Sheldon), and, of course, the Editor of CP.

STRANGE as it may seem, the word *moment* means not only (1) a minute portion of time, but also (2) importance (*of great moment*), (3) tendency to produce motion (force \times distance, Archimedes), and (4) potency to determine the center of a statistical system (product-moments, Pearson). The title of this pair of volumes, *Moments of Discovery*, is correct in the last three senses. They tell, for 83 authors from Hippocrates to Oppenheimer, plus many others who appear without speaking for themselves, how truth was pried out of nature—temporary truth, of course—just what leverage, what moment, was necessary to produce the momentous.

He who buys these thousand pages expecting them to be packed with accounts of those flashes of insight, each dated by its own "minute portion of time," will not have his expectation fulfilled; yet he may be rewarded in other ways, for this brace of books is no mean achievement, and the psychologist who goes to them is presented not only with one of the easiest and most attractive ways to gain a first perspective upon the history of science, but also with a great deal of wisdom as to how the creative mind works in science. It is true that the psychological analysis of the nature of creativity the reader has

to make for himself, for the author-editors are content as author to present facts and as editors to let the scientist himself tell what it was he did and sometimes how his thinking went on. Perhaps this reviewer can, however, guide the reader.

Is science a history of sudden insights or is it continuous hard work? It is mostly hard work, which may be punctuated with little insights, some wrong to be corrected, some right, at least for the time being. The main business of science is logic and experiment, with hypotheses for testing coming now and then by insight. The brilliant flashes are comparatively rare and often the reports of them turn out to be apocryphal, like Voltaire's story of Newton and the apple. Many of the true instances are hidden away in the reticences of the investigator or else lost from his memory. There is Descartes' dream on Monday night, 10 November 1619, when he saw how geometry could be made algebraic and was thus readied to found analytic geometry. There was Fechner's reverie in bed on Tuesday morning, 22 October 1850, when he realized—an incorrect insight with tremendously good results—that Weber's Law could be used to resolve the mind-body dilemma. And what happened in Wertheimer's mind, when he got off the train at Frankfurt in August 1910 before he reached his intended destination, to start the founding of Gestalt psychology with a toy stroboscope in a hotel room? And, for that matter, what really happened to Saul of Tarsus? Our belief in these sudden insights is easy, for they are as near as you get to magic in a natural, causal world. Cer-

tainly not all creative thinking is deliberate. Köhler's chimpanzee did indeed put the two sticks together to make a rake long enough to reach the banana, and ever after his command of technology remained advanced by just so much.

What Schwartz and Bishop tell about quick thinking are instances of the investigator's seeing a new and unusual significance in an observation. Tycho Brahe on 11 November 1572 looking at the heavens, the good old heavens so familiar to him, and seeing set in a new spot an unfamiliar brilliant star. Galileo on 8 January 1610 suddenly realizing that the three little stars that his new telescope revealed as next to Jupiter had moved since the night before, two of them from the left to the right of the planet. Yet it took four successive nights before he felt sure that these 'stars' were moons. Kepler, after "by unceasing toil through a long period of time" (17 years) during which he studied Brahe's data on the courses of the planets, finally at last conceived "the true relation of the periodic times to the orbits" on 8 March 1618, although he was not quite sure he was right until 15 May. Claude Bernard (no date for this one) noticed that the urine of the rabbits just brought from the market was clear and acid, whereas normal rabbit urine is turbid and alkaline, and he wondered why, and then formed the hypothesis (correct) that the abnormality was due to the fact that the rabbits were hungry. (Carnivora, habitually hungry, clear; herbivora, habitually replete, turbid.) Linus Pauling, who writes a foreword to these volumes, gives an instance of his own sudden thinking. (It looks, though, as if Pauling thought the title of these books should be *Instants of Discovery*, not Archimedean Moments.)

But most of scientific activity is good hard work. "Invention is not often the quasi-miraculous discovery of an individual endowed with special gifts," remark the author-editors. Newton worked long over the results of Copernicus, Brahe, Kepler, and Galileo, and had to develop the calculus to handle them before he could settle on his fundamental law. No simple falling apple did that trick. Darwin spent five years on the



PHILIP W. BISHOP

Beagle collecting data and then twenty years more working out their significance. Whether he had a quick or a slow insight after he read Malthus' *Essay on Population* is not clear. That the *Zeitgeist* was helping him is plain from the story of how Alfred Russel Wallace almost anticipated him in publication in 1858.

Now for a few items in this binary tome which will be of special interest to psychologists.

There is not much told about the *Zeitgeist's* helping or hindering. Schwartz and Bishop are not on the lookout for those relationships. They say of Harvey that, although he "was himself a product of the intellectual climate of his time, he was able further to loosen the fetters of that medieval authoritarianism which . . ." Many others than Darwin worked long in an atmosphere created by other investigators. As to hindrances, Schwartz and Bishop's whole enterprise is set to select the instances where progress did not fail.

Pauling thinks that "*Moments of Discovery* makes it possible for modern man . . . to make predictions about the future." Surely he must be wrong. That is, in the opinion of this reviewer, the common-sense view, the common-sense hope, and the common-sense error.

Even Descartes, as quoted here, would not himself venture prediction in science. A good way to refute this widely held faith would be to review this whole account to show that it is always the unexpected that is being discovered, even when the *Zeitgeist* is pushing the investigator hard, even when the trend, hidden at the time, is going to be conspicuous to the judgment of posterity.

Those who are interested in the distinction between models and theories should read about Copernicus and how Andreas Osiander, a Lutheran theologian, added a preface to *De revolutionibus orbium coelestium*, intending to placate the Protestants by "describing the work as a mathematical exercise rather than an astronomical treatise," that is to say, as a mathematical model rather than the truth. Giordano Bruno went to the stake in 1600 for holding that the Copernican model was true. Had he kept to the model he need not have died at fifty-two. In 1613 Galileo was forced by the Inquisition to accept an equivalent of Osiander's apologetics. Bush and Mosteller in their *Stochastic Models for Learning* (1955) gained for themselves the same freedom from inquisition and in the same way.

These volumes, continuously paged from the first through the second, seem to this reviewer to be very good, an introduction to the history of science for him who knows nothing much about the subject and thus for many of *CP's* readers. George Sarton, the dean of the history of science, could hardly have liked them, for these one thousand pages would have been for him too superficial as compared to the 4,245 pages (2-3 million words) of his *Introduction to the History of Science* that takes you through the fourteenth century only. You have, however, to start somewhere and *McGuffey's Aristotle* is a contradiction of terms.

Schwartz teaches biology, especially ecology, at New York University, where he has been a graduate student as well as at Columbia. He is a Fellow of the New York Academy of Science and a member of the History of Science Society. He is frequently on educational television and has written the volume on *Natural History* for the *New Wonderland Encyclopedia*, due out right

now. Bishop comes from the University of London's School of Economics and has a Yale PhD. He is Head Curator of the Department of Arts and Manufacturing of the U. S. National Museum and is a member of the American Economics Association. In other words, these men are, like Fritz Kahn and Amram Scheinfeld, dedicated to the enterprise of bridging the gap between the savants and the intelligentsia, between Sarton and you. It is an indispensable undertaking.

THE two author-editors present in English 89 excerpts of translations for 82 men (and Mme. Curie) who made momentous contributions to science. The excerpts run from one to 34 pages, an average of about 8 pages per man. Galileo gets the 34 pages. Newton, appearing twice, gets 32. Van Helmont gets less than one. The translations are excellent, and this reviewer, though a layman in history of science, thinks that the selections for an untechnical reader are very good indeed. Are they too short? What can you do with so big a subject?

The offerings are grouped under 15 topics, each anticipated by three or four pages of orientation written by the au-



GEORGE SCHWARTZ

thor-editors, who also introduce each selection with a couple of pages placing the particular item in relation to other work and describing the man. Altogether these author-editors have written over 250 pages themselves, a little more than a quarter of the two books, and their writing is good as to clarity, felicity, succinctness, and pertinence. If you read only Schwartz and Bishop's 250 pages, you would have read an excellent ac-

count (for its size) of the history of science, from the ancient Greeks to the end of the nineteenth century, but that you would hardly do, for, having read about a man and his contribution, it would surely be impossible for you to stare at his own words, untechnical and interesting, right there before you and not to sample them. No better trap for a beginning scholar could very well have been devised.

in the basic disciplines (more precisely, after the age of about eight months, when it is weaned). As the child grows older, his peers become very important as socializing agents. The role of the parents as disciplinarians is negligible, for their role is to be 'good friends' with their children and to love them. This separation of the traditional parental roles gives particular significance to the kibbutz as a laboratory for the study of the development of personality.

(3) Strong emphasis is placed on co-operation and sharing and a strong negative value on competition.

Spiro deals with the influence of these different agents of socialization, the techniques of socialization, and the content of socialization on the development of the children's personalities—with the values and attitudes to be inculcated in them. He insists that he does not deal with this particular kibbutz as representative nor does he wish to generalize from this settlement to others or to make generalizations about collective education. He aims rather to deal with the relationship between the variables of socialization and certain variables of personality—to test current theory in culture and personality and to discover "new theoretical relationships between culture and personality." The variables he chooses for analysis are the ones emphasized by studies of culture and personality and by personality theory (mainly psychoanalytic theory).

WITH regard to research methods and techniques, Spiro tries to combine the techniques of anthropological field work with various psychological techniques such as systematic observations for quantitative analysis, formal interviewing, and the use of questionnaires. It is in the use of these latter techniques that the author is weakest, leaving himself open to criticism. For instance, he quantifies the data on 'toddlers' where quantification is meaningless—meaningless because one is not sure what is being counted or whether the various acts being counted and compared are comparable. In many cases the observations were not carried on for equivalent lengths of time. Nor were the observation situations standardized;

Socializing Children in Israel

Melford E. Spiro, with the assistance of Audrey G. Spiro

Children of the Kibbutz. Cambridge, Mass.: Harvard University Press, 1958. Pp. xix + 500. \$10.00.

Reviewed by HELEN FAIGIN ANTONOVSKY

Dr. Antonovsky is Research Associate in the Bank Street College of Education in New York City, where she is working on a project that concerns mental health in the schools. She has her doctorate from Harvard University where she worked in the Laboratory of Human Development on parent-child relationships and the development of the child's personality. She was then also participating in field work in cultural groups in New Mexico. After her degree she spent five years in Israel, partly in a clinic for child guidance, partly teaching in the Hebrew University, and partly studying the social behavior of 'toddlers' in kibbutzim.

HERE we have the second of three books planned by the author covering one year's field work in a collective settlement (*kibbutz*) in Israel in 1951-52. The author says that the present volume is a more technical and scientific work than the first, *Kibbutz: Venture in Utopia*, which is an easily read, descriptive analysis of the history and structure of a collective settlement, one of several hundred such, in which about five percent of Israel's population lives. *Kibbutz: Venture in Utopia* is more descriptive than analytic. It is good background reading for the pres-

ent volume. *Children of the Kibbutz*, on the other hand, is a scientific work and the criteria used by the reviewer in evaluating this book are in these terms. The third book planned for this series will be essentially a supplement and rounding out of the present volume, presenting an analysis of Rorschach and TAT protocols obtained by the author on the children studied.

Children of the Kibbutz offers a description of socialization and education in a collective settlement in Israel with an analysis of the influences that a number of socializing or antecedent variables have on various aspects of the personality of children and adults as they grow up in this type of society.

Probably the briefest way to characterize the difference in patterns of socialization in this communal society from the patterns in the average western family is to specify three basic differences.

(1) Children live in bisexual groups in their own special houses from birth through adolescence.

(2) For the child there are three sets of significant socializing agents: parents, nurses and educators, and peers. For the very young child the parent surrogates—nurses and educators—have the main responsibility for training the child

one observer, the author, was a more or less neutral observer, whereas the second observer, his wife, played an active role as nurse substitute in some of the groups. There was, moreover, no attempt to measure reliability of the observations. Since the author himself often expresses an awareness of the weaknesses of his quantitative material, one wonders why he did not discard this technique, for it adds little to the stimulating and insightful descriptive materials.

Many of the questions asked of the parents in the formal questionnaire are too general and direct to provide the needed confidence in the answers, "Did you rebel against your parents?" "Should children blindly obey their parents?" "Do your older children admire you?" "Are you the most important influence on your child?" The answers to such questions were taken at face value. Does Spiro not regard them as open to both conscious and unconscious distortion by the respondents?

In contrast to the above criticisms, the ethnographic descriptions, based on such procedures as participant observation and informal interviewing, are very good. They give the reader a meaningful and clear impression of the process of collective education.

In presenting and analyzing the data on early patterns of socialization, Spiro utilizes, for the most part, the general framework of Whiting and Child. The reviewer has the impression that the data were collected without this framework in mind—at least not explicitly—since in many cases there are not enough data to make the analysis meaningful. It would have made more sense and would have been clearer if the author had dealt with the data descriptively rather than analytically.

In the last section of the book Spiro describes various personality characteristics of the children and adults who grew up in this system of collective



MELFORD E. SPIRO

education and tries to relate them to various types of childhood experiences. Here again the analysis is often not convincing, and in some cases the reasoning is circular, as it is in the sections on the need for approval and superego development. The section on emotional adjustment is, on the other hand, much clearer.

Probably the greatest weakness of the book is its lack of theoretical and analytical coherence. As a descriptive ethnography it is very good.

The book is well worth reading by those interested in personality theory and in the general area of culture and personality. It does not, as its author hoped, rigorously test current theory in this area, but it should, nevertheless, prove useful as a source of significant hypotheses. The description and analysis of this pattern of child rearing raise important theoretical questions and their discussion here should serve as a spur to further research. The description is also of importance to those interested in a unique method of socialization and education as practiced in a modern, western communal society.



All intellectual work is in fact physically exhausting. There may be no such thing as brain fog but thinking is done with the whole body, and sixteen hours at a desk composing words or notes is an ordeal that few men not trained to it could withstand.

—JACQUES BARZUN

After the Rorschach, What?

Emanuel F. Hammer (Ed.)

The Clinical Application of Projective Drawings. Springfield, Ill.:

Charles C Thomas, 1958. Pp. xxii + 663. \$13.50.

Reviewed by HAROLD H. ANDERSON

who is now Research Professor of Psychology at Michigan State University. He has his doctorate from the University of Geneva (la Suisse), has been at the Universities of Iowa and Illinois for long years each, and at this present site since 1946. With his wife he has edited *Introduction to Projective Techniques* (Prentice-Hall, 1951), and will pretty soon have edited *Creativity and its Cultivation* (Harpers, August 1959).

PROJECTIVE drawings, explains the author in his preface, have a secure niche in the projective battery of the clinical psychologist. They and the Thematic Apperception Test are the most frequent supplements to the Rorschach. The aims of the author are to assemble and integrate into one volume the contributions of a vast body of literature on the projective significance of drawings and to treat the material primarily from a clinical point of view as an economical diagnostic method and an adjunct to therapy. "The empirically-based observations that form the backbone of this book are offered as hypotheses which, for the most part, still await experimental investigation and verification."

Hammer is Head of the Psychology Unit of the Psychiatric Clinic in the Court of Special Sessions, and also Psychologist of the Child Guidance League, both of New York City. He was formerly Senior Research Scientist at the New York State Psychiatric Institute and Director of Intern Training in the Lynchburg State Colony, Virginia. In this book he is the author of 300 pages that comprise 12 of the 28 chapters. The jacket says the book is by Hammer "with the Collaboration of 11 AUTHORITY

TIES in the Field," naming the eleven on the jacket. There are actually 15 additional contributors. Among the four omitted on the jacket is the author of the only chapter on research; one of the four is also omitted from the list of contributors inside the book. Hammer is named alone on the title page as author, but on the outside cover as editor.

In addition to Machover's Draw-a-Person (DAP) technique and Buck's House-Tree-Person (H-T-P), the book treats the Draw-a-Family procedure, Harrower's Most Unpleasant Concept test, Kinget's work with the Wartegg Drawing Completion Test, a modification of Abram's Draw-a-Person-in-the-Rain, Levy's Draw-an-Animal, Caligor's Eight-Card-Drawing-Test, chromatic drawing, doodles, and Naumberg's use of Art in therapy. The writing is primarily for clinical psychologists and trainees.

Hammer has crowded these techniques into a huge book of glossy pages. I say crowded because, for example, in one sentence, that begins on p. 408 and, five pages later, ends on p. 412, Hammer presents six drawings, that occupy also parts of two additional pages. Kinget's Scoring Blank and her Interpretation Blank, which offer one of the rare opportunities in the book for quantitative scoring and structured interpretation, are literally crowded into less than half a page with type too small to be practically useful.

As for integration, the author explains in his last chapter that this goal is yet to be achieved. The book is full of metaphors, but it is difficult to find for it a backbone, a goal, or a perspective.

THE volume reflects the state of clinical psychology today. In a broad sense its limitations are not uniquely the limitations of the authors, for the book is largely written by dedicated people, whose dedication, in spite of their own protestations of devotion to science, nevertheless limits their view. In Rorschach terminology, this is 'Dd' psychology, bent on finer and finer analysis and refinement of one's view of the 'patient.' There are many mentions of the words *dynamic* and *process*; yet the basic approach is to an understanding of the person as if he could be represented by

himself, in testing room, *in vacuo*, or in pictures.

The concept of stress and stressful situations is introduced but not elaborated, not even in the chapter on Draw-a-Person-in-the-Rain. Stress is an old concept for physicists and engineers, and more recently a useful concept for Selye in medicine in his study of physiological limits of organisms. But stress is new to psychology, and strange. It is neither stimulus nor response. It is like a bridge; it has two ends. Or it is like gravity or love: the relating is two-way simultaneously.

Hammer knows that people do not live in a vacuum and says so in many places. His book, however, is directed to examination of the *person*, to the diagnosis and treatment of one end of the inseparable relating. The environment is here admitted and there ignored. Well—it took a Newton to think up gravity and a Selye to introduce stress into physiology. Psychologists, too, may in time figure out what they mean by dynamic and by internal consistency.

Other basic topics recur in the book without critical examination; e.g., the superego, sex, and the unconscious. Item: "Bender concluded from her study of animal drawings that children tend to draw aggressive animals if they suffer from a severe superego which leads to a fear of the devouring animal."

Filled with metaphors, this book is an American allegory without a plot. In it the superego becomes a horrible monster. It browbeats, devours, scorches, burns, frightens people, many people, most people, and mostly when they are very young. Clinical psychologists have somehow missed reading *Society as the Patient* (1936), written by L. K. Frank, the one who in 1939 introduced the term *projective techniques*. Nor is anyone waiting for Saint George to come along, slay the dragon, and thus remove at least some of the clinical problems. No one has even thought of it—not in this book.

So? Projective drawings assist the clinical psychologist in understanding the patient's feelings, how it feels to be browbeaten, scorched, burned, frightened, regardless of when it was done or by whom.

The Search for Universal Factors

Yrjö Ahmavaara and Touko Markkanen

The Unified Factor Model: Its Position in Psychometric Theory and Application to Sociological Alcohol Study. Helsinki: Finnish Foundation for Alcohol Studies, 1958 (distributed by Almqvist & Wiksell, Stockholm, Sweden). Pp. 187. Sw. kr. 18:—.

Reviewed by JUM C. NUNALLY

who is Assistant Professor at the University of Illinois, where he conducts research, mostly in the Institute of Communications Research, and teaches in the Department of Psychology, mostly multivariate analysis. He has a PhD from the University of Chicago and acknowledges the stimulus there of William Stephenson and Leon Thurstone. He has published variously on communication and semantics, selection and factor analysis, mental health and psychotherapy. He is the author of a new book which is called *Tests and Measurements: Assessment and Prediction* (McGraw-Hill, 1959).

THE monograph consists of two separately authored parts. The first part, authored by Ahmavaara, presents a "unified factor analysis model." In the second part, Markkanen applies the model to factor-analysis studies of the consumption of alcohol in Finland.

The monograph is addressed to a very important problem—the comparison of factors found in different samples of persons. Beginning with Spearman, the dream of (most) factorists has been to find the major *invariant* dimensions of individual differences. Pushed to the extreme, the requirement of *invariance* asks that the same factors occur in *all* samples of persons. Conceivably the same factors (or at least some of them) would occur among Eskimos, Watusi, French school children, and even among American psychologists.



PSYCHOLOGY for BETTER LIVING

By LYLE TUSSING, *El Camino College*

This book is concerned with the unceasing efforts of man to live a full, stable, happy life in a world complicated by the advance of the atomic age and the dynamic changes it has brought to our complex environment. If we are to survive, the need for self-understanding must serve as the essential prerequisite to world understanding. And if we are to understand ourselves, we must utilize our ability to think logically and make decisions in order to evaluate and solve our personal problems. Tussing shows how will power, common sense, and the scientific method of problem-solving can be used to advantage. He also points out the necessity for securing a sound philosophy of life.

In planning better living patterns, the author fully discusses learning principles, methods of concentration, and retention of ideas. Later portions of the book discuss personality traits, including specific methods and rules for overcoming shyness; the development and control of emotions; mature leadership and adjustment mechanisms; mental illness and how to avoid it; and choosing a marriage partner. The book closes with a discussion of the pursuit of happiness, the future relationship of man to man, and mental health and the future. 1959. Approx. 448 pages. Prob. \$4.95.

Automatic Teaching

The State of the Art

Edited by EUGENE GALANTER, *University of Pennsylvania*. This volume is based on a symposium treating the automatic teaching of verbal and symbolic skills. 1959. *In Press*.

10,000 Careers

By ROBERT L. THORNDIKE and ELIZABETH HAGEN, *both of Teachers College, Columbia University*. Presents the first correlation of results of occupational and guidance tests with actual career performances of 10,000 men over a span of 13 years. It is by far the most comprehensive follow-up study of aptitude test results that has ever been carried out. 1959. 346 pages. \$8.50.

The Motivation to Work

By FREDERICK HERZBERG, *Western Reserve University*; BERNARD MAUSNER, *University of Pittsburgh*; and BARBARA SNYDERMAN. Introduces a new theory of job motivation, job satisfaction and job attitudes. 1959. Approx. 184 pages. Prob. \$5.00.

Modern Organization Theory

A Symposium of the Foundation for Research on Human Behavior

Edited by MASON HAIRE, *University of California, Berkeley*. Eleven distinguished scholars have contributed to this symposium volume, which treats the latest and most significant research and theory related to organizational behavior. 1959. *In Press*.

Elementary Decision Theory

By HERMAN CHERNOFF and LINCOLN E. MOSES, *both of Stanford University*. Introduces statistics as the science of decision making under uncertainty. 1959. 364 pages. \$7.50.

Design for a Brain

Second Edition

By W. ROSS ASHBY, *Department of Research, Barnwood House, Gloucester*. 1959. *In Press*.

Individual Choice Behavior

By R. DUNCAN LUCE, *University of Pennsylvania*. 1959. 153 pages. \$5.95.

Send for examination copies

JOHN WILEY & SONS, Inc. 440 Fourth Avenue, New York 16, N.Y.

Unfortunately (maybe), it is not possible to apply *all* tests to *all* persons and in one grand analysis determine the invariant dimensions of individual differences. Instead we must work at it piecemeal, applying some tests to some people and comparing the factors with those found with the same or different tests applied to the same or different people.

If the same sample of persons is used in different studies, direct comparisons can be made among the factor results. The more usual situation, however, is that the same, or some of the same, tests are administered to different samples of persons. For this circumstance there is as yet no rigorous means for comparing the factors obtained from different analyses. Comparisons are usually subjective, eventuating in statements like "They look the same," and much confusion and controversy have resulted. Ahmavaara on several previous occasions proposed a method for the matching of factors found in different samples, and in the present monograph with Markkanen the method is applied to an empirical study.

The best way to read the monograph is to start with Chapter 4 in Part I. The first three chapters, comprising nearly half of the entire monograph, are interestingly unrelated to the main issue presented in the succeeding pages. The first three chapters give a running description of a little bit of everything in psychometrics, including Guttman scaling, Rashevsky's mathematical models, Thurstone's 'box problem,' and so on. The account is sketchy, will provide little that is new to people in psychometrics, and will only confuse readers who are new to the problems. In addition, the first part of the monograph is written in a quaint English that is sometimes difficult to read. (In contrast, the second part of the monograph is written in crisp English.)

THE proposed method for studying the invariance of factors is spelled out in Chapter 4. The scene is set when the same battery of tests has been administered to two different samples of subjects and the results of each have been factor-analyzed separately. (The factors

can be either rotated or unrotated, either oblique or orthogonal.) To study the invariance of factors in the two samples, a transformation matrix is sought such that the factor loadings (or "specification-equation" weights) from one study are made the same as those from the other study. The authors claim that if such a transformation matrix exists, it demonstrates the invariance of factors from one analysis to another. Further, they recommend that such transformation matrices be applied in practice to clarify factor rotations and interpretations.

Because the whole monograph centers around the "transformation analysis," a number of points about the method should be considered. First, the method is not new: British and American psychologists have worked on similar methods for over ten years and have sought remedies for some loopholes not even mentioned in the monograph. (For some of the history of these developments see R. B. Cattell and A. S. K. Cattell, Factor rotation for proportional profiles: analytic solution and an example, *Brit. J. Stat. Psychol.*, 1955, 13, 83-92.)

The second important consideration about the method is that, even though one can write the equation to find a transformation, no exact transformation is likely to be found. Consequently, the factorist is thrown back on seeking an approximate transformation—a 'least-squares' solution is the one most often recommended. Ahmavaara and Markkanen do not make the problem of statistical approximation explicit and leave unclear what they would do or recommend.

Third, in order to use the method in practice, it would be necessary to find a good approximation by the transformation method. The results which the authors obtain are rather disappointing. (See the numerous large discrepancies in Table 6, p. 122.) A previous study by Ahmavaara of a broad range of factors of human ability produced equally disappointing results (*On the Unified Factor Theory of Mind*, 1957; *CP*, July 1958, 3, 184f.). Either the method is improper or the factors are less than invariant.

Fourth, although the transformation

method has some common-sense backing, it is not, as presently conceived, a complete solution to the matching of factors. It fails on a crucial counter example: there is nothing to prevent seeking a transformation of some of the factors in an analysis to other factors in the *same* analysis. It is not hard to compose examples of this kind in which the matching is, by current standard, reasonably good. This raises some serious questions about the method.

In spite of the importance of what the authors are working so hard to accomplish and the notable contributions which they have made in previous publications, the present monograph does them little justice. Much of it is irrelevant to the main issue; the central problem of "transformation analysis" for the matching of factors is not presented adequately, and the empirical demonstration of the method is disappointing.

The Sounds that You Hear

John R. Pierce and Edward E. David, Jr.

Man's World of Sound. Garden City: Doubleday, 1958. Pp. 287. \$5.00.

Reviewed by KARL D. KRYTER

who, with a PhD from the University of Rochester back in the days of Elmer Culler, has worked on research in Harvard's Psycho-Acoustic Laboratory, at Washington University in St. Louis, in the Air Force's Human Resources Research Laboratories, and now with Bolt, Beranek and Newman, Inc., in Cambridge, Massachusetts. All along it has been speech and communication, signal and noise, that has concerned him. Right now he is on the psychophysiology of complex sounds and on sleep and noise.

PIERCE AND DAVID are research scientists at the Bell Telephone Laboratories. Pierce is responsible for direct-

ing the research program of that organization in Electrical Communication, and David is Assistant Director of Visual and Acoustics Research. (Pierce, by the way, is also the popular science-fiction author, J. J. Coupling.)

The art of communication engineering has been developed by an admixture of research in the fields of electronics, acoustics, psychology, and physiology. Each pertinent advance in electronics and acoustics created a need for information in psychology and physiology; and knowledge of man's auditory behavior often suggested new and novel approaches to the design of communications equipment.

Man's World of Sound is an account of where we stand today with respect to acoustical, psychological, and physiological concepts that are relevant to communications engineering. As such, the title is somewhat misleading, for less than a third of the book is concerned with man's world of sound; the remainder is about man himself and about electronic instruments and concepts for handling information.

Perhaps the most efficient way to convey the scope and flavor of the book is through the chapter titles: *The Proper Study of Mankind*; *The Power of Sound*; *Waves, Frequencies and Resonators*; *The Source*; *Giving Form to Sounds*; *The Acoustic Nature of Speech*; *What Do We Hear?*; *Ears to Hear With*; *Nerves and the Brain*; *Defects of Speech and Hearing*; *With the Speed of Thought*; *Intelligibility*; *Quality and Fidelity*; *Automata and Talking Machines*; *Efficient Communication and Intelligent Machines*; *The World Is All Before Us*.

In spite of the variety in its contents, the volume hangs together fairly well. It is kept from exploding by the authors' choosing only the highlights and major features of the more significant ideas and data related to their theme. Credits, references, confusions, and arguments—except in the more important and critical cases—are forgotten in favor of presenting as straightforward and readable a book as possible. Nevertheless there is no compromise in the accuracy of the facts presented and the coverage of the book in view of its relative brevity is impressive. There was

only one chapter, the first, that I felt could be trimmed if not removed. The title of that chapter, *The Proper Study of Mankind*, is overambitious for the contents and the important thoughts that are there appear again in later chapters.

Part of the cohesiveness of this book is also attributable to the literary style of the authors. They use anecdotes, clever analogies and, as a twist, an editorial 'I' that makes the text less formal than usual for material of this type. Pierce's and David's interest and excitement in the subject matter, and in science in general, are generously revealed in all parts of the book. My hunch is that in spite of its informal, personal style, the novice will find the going tough—there is less redundancy here than in many textbooks.

THE authors occasionally appear a bit biased and arbitrary. For example, we find on page 113 the statements: "We can only conclude that for sine waves, at least, 'equal' musical intervals do not represent equal intervals of subjective pitch. This baffled me to the extent that I nearly left the mel scale out of this book. . . . I am now inclined to believe that the mel scale reflects a 'place' mechanism in the ear . . . , while the scale of musical pitch is associated with another, a time-comparison phenomenon." The reasoning here is not clear or justifiable. I believe there is no evidence to indicate that the equal musical intervals represent equal pitch intervals. It is reasonable, however, to suggest that the character of several simultaneous tones from a musical instrument, or an oscillator for the matter, and the subjective similarity among like chords (or like musical notes with their rich harmonics), regardless of the octave in which they are played, are partially the result of an ability of the auditory system in time-comparison as distinct from the 'place'-pitch phenomenon. Perhaps this similarity among like chords or musical notes is what Pierce and David had in mind when they spoke of 'musical pitch.'

With reference to the ability of persons to discriminate among speech sounds, we find on page 128 the fol-

lowing: "It is for this reason that I cannot take seriously simple-minded experiments which purport to show how many pitches or levels of sound we can recognize." It appears that 'I' must have missed the meaning of the studies in question, and perhaps overestimated the problem of evaluating the cues used by people for recognizing speech sounds. The 'simple-minded' experiments to which they referred (I presume, since no reference is given) demonstrated an important perceptual fact about hearing—namely, that the number of possible discriminations among sounds on the basis of the attributes of pitch and loudness alone is relatively small, and that this ability is not improved appreciably with learning. Irwin Pollack and Lawrence Ficks (1954) found that, for a significant amount of auditory communications, the signal must be coded on a number of dimensions and combinations of dimensions. The number of discriminations found possible from relatively simple coding is not inconsistent with the perceptual demands placed upon man by the number of speech sounds used by a given language.

These troubles are minor. They detract little from the text. The outstanding value of this book is that it brings together material from such a wide variety of sources. No other does this in quite the same way. Other treatments of the same general area (G. A. Miller's *Language and Communications*, Fletcher's *Speech and Hearing*, and chapters by Licklider and Miller in S. S. Stevens' *Handbook of Experimental Psychology*) are more detailed, informative, and authoritative, but each omits some of the parts included by Pierce and David (although they also include items not reported on by Pierce and David). This new book will serve the cause of speech communication very well—both as a starting point for the intelligent novice and as a succinct, fairly comprehensive 'refresher' for the more advanced student, researcher, and engineer.



*If we knew how our bodies are made,
we would not dare to move.*

—GUSTAVE FLAUBERT

Aftereffects: Figural and Negative

Peter McEwen

Figural After-Effects. (British Journal of Psychology Monograph Supplements, XXXI.) New York: Cambridge University Press, 1958. Pp. 106. \$4.50.

Reviewed by JAMES J. GIBSON

Dr. Gibson is Professor of Psychology at Cornell University, although this past year he has been at the Institute for Advanced Study in Princeton. Before he went to Cornell he came under E. B. Holt's influence at Princeton and Kurt Koffka's at Smith College, where he discovered that a world with optical curvature induced in its spine straightens itself up if you give it time. He has been wanting to straighten crooked things ever since, and in this review indulgent CP has let him first praise McEwen and then be himself.

PERIODICALLY, a field of research gets so complicated that the active investigators themselves lose their bearings. Anyone who then intervenes by surveying the facts without adding new ones deserves the thanks of all. When the survey is balanced and critical, the service done is great, and, when the field surveyed is disorderly and beset with experimental difficulties, the achievement is impressive. McEwen's monograph—part of a doctoral thesis from Queen's University, Belfast—is a survey of this kind. Nothing yet published, out of some 180 titles listed, matches it in objectivity or clarity. The Japanese work on figural aftereffects is not covered, but the brief review by M. Sagara and T. Oyama in the *Psychological Bulletin* (1957, 54, 327-338) is referred to.

The experiments of Köhler and his collaborators, beginning in 1944, together with the theory of cortical satiation derived from them, make a highly intricate chapter in psychological research. I have sometimes suspected that not all converts or critics of the theory

understood it. McEwen does, and he can follow the reasoning from test figures to electrical fields with sympathetic but critical insight. He can therefore order the evidence from the dozens of experiments that have followed with clarity and in detail. He shows that there are deep contradictions in the interpretation of the facts and formidable difficulties in the experimental methods used. But he demonstrates that the concept of neural satiation has stimulated so many experiments in such a variety of directions that no one can afford to disregard it despite its "failure to come to terms with the known physiology of the nervous system, or to provide any indication of the way in which cortical currents are retransformed into neural impulses." The alternative theory of Osgood and Heyer is considered fully but, as McEwen points out, it has failed to inspire any original research even in its own authors.

It appears that aftereffects in bidimensional visual perception, not to mention visual depth or kinesthesia, are not describable by any general rule yet formulated. They are still a puzzle. The displacement of a phenomenal contour from the locus of a prior contour in the visual field, while covering many, does not cover all the facts. It is not itself a simple rule, because the degree of displacement as a function of distance from the locus is in doubt, and there are some constrictions, expansions, or distortions of contour which do not fit the rule, as Köhler and Wallach themselves recognized. As long as an empirical rule is lacking, a theoretical formula remains tentative.

It is unfair to criticize an author for

not having written a different book. Nevertheless, since this is a field in which I myself am involved, my discussion of McEwen is inevitably tied to my own beliefs. I wish he had clarified the distinction between figural and negative aftereffects, as he might have done. Instead, he asserts that figural aftereffects were "discovered by Gibson but named by Köhler and Wallach," and this comment, in my opinion, prolongs a confusion that ought to be cleared up.

If McEwen had given space to the theory of a negative aftereffect as simply the consequence of an adaptation process (Gibson, *Psychol. Rev.*, 1937, 44, 222-244), he could not have failed to see the difference. But this paper of mine is not listed in his bibliography, nor is it cited by most of the investigators of figural aftereffects, from Köhler and Wallach to the present. Thus a useful experimental method has, I think, been obscured.

IT has been widely assumed for the last 15 years that negative aftereffects are explained by figural aftereffects, despite the fact that the former include effects which are *dimensional* in character, not figural—phenomena of color, temperature, visual and tactual motion, kinesthesia, and the arrangement of objects in space. The existence of such effects can hardly be questioned, nor the fact that they cut across the traditional distinction between sensory and perceptual phenomena. One proof that perceptual qualities yield to adaptation comes from the experiments with prismatic spectacles, which have been going on at Innsbruck under Erimann and Ivo Kohler for many years, even before I began to use them in 1929. The normalization of the properties of the phenomenal world after wearing prisms—its curvature, density, and color, for example—has been well established, and it was in order to measure the adaptation (or the negative aftereffect) that I began to work with curved lines. Even more generally, the normalization of psychophysical variables has been demonstrated in experiments concerned with 'adaptation-level,' as Helson has called it. When Wolfgang Köhler and Hans Wallach began to work with lines and

figures, they had a different purpose. They wanted to discover the cortical substrate of visual form. My interest had been to discover what properties of visual stimulation will induce what properties of visual form.

It is no wonder that Köhler and Wallach wanted to subsume the negative aftereffects of curvature and tilt under figural aftereffects. Any theorist hopes for parsimony. The effort to separate the two effects by subsequent experimenters is described by McEwen; it was half-hearted and ended in a stalemate. But the unfortunate result of this failure has been that two different methods of research have been confused, and that one has been neglected.

An experimenter can, first, study the phenomenal distortion of an object on one side of the fixation point relative to the identical object on the other side. Or, second, he can study the alteration of some quality of an object in time relative to its proper stimulus. The experimental methods are different and they point in different directions. The first has neurological implications, the second psychophysical ones. The first reveals a change in perception, the second a change in the relation of stimulation to perception. The difference in the kind of facts sought is connected with an important issue in psychological theory—whether to start with the figure-ground phenomenon as a basis for research, to consider a figure as such, or to start with the psychophysical experiment as a basis for research, to consider the dimensions of variation of a figure.

Aftereffects in perception are not only useful indicators of some sort of cortical activity but also, and primarily I think, of a change in the power of a stimulus to evoke perception. Any aftereffect implies a change in psychophysical correspondence. The negative aftereffect is a shift in the quality evoked by a stimulus in the direction opposite to that of the adapting stimulus. It is this *relation* between the retina and the brain, not just the process in the brain, which the investigators of figural aftereffects have failed to recognize.

The relation between the brain and visual experience is a persistent mystery that has always captured the imagina-

tion of psychologists. A method which promises facts about this relation is bound to produce a large body of research in the course of fifteen years. McEwen has fairly stated the outcome of all this work, and the reader may judge whether the mystery has been resolved. The relation between the stimulus and the visual experience, on the other hand, arouses less interest, but, until it is established, the role of the brain in perception cannot be isolated for study. The failure to realize this point is the only weakness in McEwen's otherwise useful critique.

Insulin Shock for Schizophrenia

Manfred Sakel

Schizophrenia. New York: Philosophical Library, 1958. Pp. vii + 335. \$5.00.

Reviewed by LESLIE PHILLIPS

who, for ten years more or less, has been Director of Psychological Research in the Worcester State Hospital in Worcester, Massachusetts, and Professor of Clinical Psychology at Clark University. He is a clinical psychologist and in 1953 he published with J. G. Smith a book called Rorschach Interpretation: Advanced Technique.

THIS book is concerned with three major but divergent aspects of the schizophrenic process. First, the tools of descriptive psychiatry are outlined, highlighting the methods of psychiatric classification and providing instructions for conducting a 'mental status' (diagnostic interview). There then follows a discussion of psychopathology, with an emphasis on etiology in schizophrenia. A final section, approximately one-half of the book, is devoted to a detailed set of instructions for carrying out insulin coma treatment. This method of treatment was first introduced by the author in 1927.

That part of the text concerned with descriptive psychiatry duplicates what is

already available in a number of manuals and is, in any case, too restricted to constitute an outline of schizophrenic symptomatology that would serve as a guide for general psychiatric practice. Of more general interest is Sakel's theoretical position on the nature of schizophrenia. Schizophrenia, he says, is a physical disease to be considered "as the end product of a deviation from the phylogenetically imprinted pattern of the nerve cell in its response to external or internal stimuli," although its manifestations occur only in the psychological sphere. He believes that no one type of person is more prone than another to develop this mental illness. Particularly surprising is the Lamarckian position which he takes on the nature of personality. He says that "each man is not only the product of millions of years of developmental progress, but also the product of millennia of psychological imprints acquired little by little from generation to generation which are molded as the final so-called 'personality.'" Nowhere does he report on the literature dealing with personality factors and psychiatric disorder. Nor does he discuss the extensive sociological and anthropological data which have accumulated about the schizophrenic syndrome.

It is illuminating to compare Sakel's theoretical position with that of other writers in the field, specifically with Bellak and Arieti, who have recently published books on schizophrenia. Bellak defines this disorder as a severe ego disturbance which may be the result of any number of somatic or psychogenic etiological factors in various combinations. Arieti believes that "schizophrenia is a specific reaction to a severe state of anxiety, originating in childhood, re-experienced and increased in some later period of life." He hypothesizes that this anxiety state influences the development of the ego and leads to an impairment of the abstract attitude.

Because these writers differ on the fundamental nature of the schizophrenic syndrome, they differ also on the preferred mode of treatment. For Sakel, insulin coma therapy not only is the treatment of choice, but the only true cure for this disease. Indeed, he claims that patients who recover through other

mean, have been misdiagnosed. In contrast, many psychiatrist investigators consider that all physical methods of treatment, including insulin coma, are at best merely ameliorative. Arieti, for example, believes that the effect of the various shock treatments is simply to reduce the individual's anxiety level by impairing the function of the frontal lobes. As a matter of fact, the recent publication edited by Bellak gives a more adequate and satisfactory description of insulin coma treatment than does this present volume by Sakel.

Bellak has provided also a comprehensive review of the literature in this area, a feature which the present work lacks.

In general, Sakel's book is dated in its theoretical position and is far too limited in its coverage to be considered a generally useful textbook on schizophrenia. Its primary function would seem to be that of a historical reference, one which expresses the views of a man who pioneered in one of the major modern approaches to treatment in the psychiatric disorders.

grand sweep of the effort, for the author attempts to correlate such diverse and difficult matters as fact and value, electronic computers and human brains, information, entropy, and cosmology, quantum mechanics, mutations, and memory, and symbolic logic, cybernetics, and semantics, all between the covers of one book. He is overwhelmed by the breadth and depth of scholarship required for such a work; no one man is adequate to the task of writing or reviewing such an effort. The dilemma is well delineated by the physicist, Erwin Schrödinger, when he says: "The isolated knowledge obtained by a group of specialists in a narrow field has in itself no value whatsoever, but only in the synthesis with all the rest of knowledge and only inasmuch as it really contributes in this synthesis something toward the demand, 'who are we?'"

Unfortunately the results of such integrative efforts have not been too helpful in terms of Schrödinger's challenge, and here is where discouragement enters the picture, for it seems doubtful that Reiser has been more successful than the others. In the process of simplifying for the sake of unification, there arises distortion which suggests that the desired synthesis is not possible. The logical positivists, for example, tried to gain unification via physicalism, but their reductive error cut away much of the legitimate content of the complex fields of the biological and social sciences. Reiser gains his unification via the analogy of the organism but in the process adds too much. This 'something-more' error is most obvious when he attributes lifelike qualities to atoms and when he elaborates his social ethic. He insists on such concepts as a world brain and an electromagnetic society. At this point we see most clearly the familiar overextension of a system, when the firmer foundation laid in his panoramic view of the formal aspects of science begins to sag under the weight of verbal adornment. Surely Reiser's effort would have carried greater impact had he been willing to stop on page 356. His last three chapters, which deal with the good society, are essentially polemical; they do not compare favorably with the more expository treatment of the first ten.

Integrative Confusion vs. Fragmentary Certainty

Oliver L. Reiser

The Integration of Human Knowledge. Boston: Porter Sargent, 1958.
Pp. 478. \$8.00.

Reviewed by JOSEPH R. ROYCE

Dr. Royce is Associate Professor of Psychology at the University of Redlands in California. He is an experimental psychologist with a bias toward 'generalism,' a bias that was begun when, working at the University of Chicago for a PhD under the stimulus of L. L. Thurstone, he found that he needed to spend half his time studying biology and mathematics. He is now working on a book that might be called The Encapsulated Man, who ought—such is the presumption that this review would create—to be freed. For CP he has reviewed Ahmavaara's The Transformation Analysis of Factorial Data (Feb. 1956, 1, 50-51).

THIS book by philosopher of science Oliver Reiser, Professor of Philosophy at the University of Pittsburgh, is offered in the spirit of synthesis of the specializations and fragmentations of modern knowledge. It is a daring and provocative book. It weaves in and around the various compartments of scientific knowledge with breathtaking verve and dynamic quality—for we do not get the usual piecemeal presenta-

tion, but rather, a full-fledged systematic position, a scientific world-view.

The major features of Reiser's conceptual scheme are these. His universe is infinite, both temporally and spatially. It is cyclic and creative, having available a pool of energy from which various manifest forms emerge via evolution. It is pantheistic, but God is depersonalized under the concept of Cosmic Imagination. The world and its components are organismic. Isomorphisms exist between the various gestalts of emergent evolution. Field theory is a unifying concept at all levels from atom to man. This scientific *Weltanschauung*, along with its logical and epistemological foundation, serves as a synthetic base from which Reiser develops a social ethic. Social integration in his 'good society' calls for a unified symbolism (Jungian archetypes), a planetary semantics (Bliss' semantography), a world philosophy (scientific humanism), and a planetary democracy (world government).

The reviewer is impressed, overwhelmed, and discouraged by this book. He is impressed by the magnitude, the

The shortcomings of the last fourth of the book point to what is perhaps the crux of the issue of generalist versus specialist. While we cannot expect generalist efforts to provide the same depth of scholarship which is possible when the author restricts his book to any one aspect of his total effort, we do have a right to expect him to be responsible in selecting the components which he is trying to integrate. For instance, Reiser might have changed his title to *An Integration of Scientific Knowledge* or, better yet, have confined himself to integrating those aspects of science which are well established, not stopping to fill in the lacunae of our ignorance with questionable theorizing. (For one thing he spent too much time speculating on the implications of the non-firm findings of parapsychology.) Some speculation is necessary in an effort of this type; the integrator must go beyond what is demonstrable; and, further, it is customary to allow the philosopher some speculative license. Nevertheless, using both hard and soft data as bases from which to make one's theoretical leaps renders the integrator's total performance dubious. That is unfortunate. Reiser's book is too exciting to be cast aside because of its wild moments.

PERHAPS the book's greatest value is heuristic. The author pulls together very unlikely components in novel and intellectually exciting ways. He has a rich and fertile mind and many high moments. His treatment of symbols and semantics, for example, is one of the outstanding chapters of his book (Chap. III) and will be of special interest to psychologists. Mathematically oriented psychologists can cogitate over his suggestion that multivalued formal systems have been untapped in human thought and all psychologists will find Chapter VIII, *Scientific Method in Physics and in Psychology*, of interest.

Certainly the generalist should be encouraged, whatever shortcomings are inevitable in his enterprise, for civilization derives its vitality quite as much from the broad insights of the generalist as from the depth probings of the specialist.

The Belief in Communism

Hadley Cantril

The Politics of Despair. New York: Basic Books, 1958. Pp. xv + 269. \$5.00.

Reviewed by PHILIP E. CONVERSE

who is Study Director of the Political Behavior Program at the University of Michigan's Survey Research Center, which is also where he got his training. Four of them there are working now on an analysis of the formation of opinion in the last three Presidential elections in the U.S. (Wiley will publish it.) It was seeing the confusion of French politics while he was at the Sorbonne that got Dr. Converse started on trying to clarify the nature of this kind of confusion.

THIS book reflects its author's abiding interest in the problems of social movements. Dr. Cantril, long a leading figure in public opinion research and currently Chairman of the Board of The Institute for International Social Research at Princeton, proffers the volume as "a more valid and inclusive systematic formulation" than that which he developed nearly twenty years ago in *The Psychology of Social Movements* (1941).

The movement under consideration consists, in this case, of the heavy Communist voting during the postwar period in France and Italy. Much of the book is devoted to protocols that capture bitterness felt toward 'the system' by two types of Communist voters: militant Party members and the more numerous nonmember "protest voters." In view of sensitive political conditions which threaten candid reporting, the collection is in itself a testimony to the skill of Dr. Cantril and his associates in the field. Two studies of attitude change among Communist voters add scope to the volume, the first a matter of artificial exposure to varying propaganda favorable to the United States,

the second a matter of 'natural' change wrought by the 1956 Hungarian revolt.

The core theory employed springs from Cantril's past work, while showing a new debt to treatments of perception by Ames and, less directly, by Brunswik. Gone is explicit dependence upon a 'frame-of-reference' construct; in its place is the matrix of assumptions, based on experience, which underpins the individual's 'reality world' and endows current percepts with significance. New also is an analysis of "ingredients required for faith," where faith is a belief in "the worthwhileness of the values that keep our reality systems from falling apart."

THE proposition that behavior is best understood when the *unique* reality world from which it issues is understood serves to justify the extensive presentation of individual case studies and, presumably, the infrequent use of bivariate tables. Such emphasis upon the personal and the unique may be helpful in drawing the naive reader to an acceptance of the unfamiliar perspectives of workers in the Old World; but it does not seem the most efficient approach to the political vistas of the Communist militant or, for that matter, to any phenomena which depend upon the sharing of a common perspective across large segments of a population, as we take to be the case with social movements.

This discomfort is compounded by a larger feeling that Cantril uses the theoretical formulation more to organize description of the reality world than to predict sequences of events—which is not to say that the theory lacks explanatory potential. Indeed, two primary theses hold (1) that the reality world is produced by past experience, and (2) that current perceptions are biased by expectations within this world. These propositions receive, however, but inadequate attention as the author pursues his description of reality world content.

The first thesis lends itself to test because individuals undergo similar life experiences, a matter which seems critical to social movements. Many facts ascertained about respondents consti-

tute powerful summaries of these patterns of social and economic experience. Thus we become interested in the degree to which variation in the reality world follows variation in such past experience. The individual cases presented, along with occasional explanatory asides, suggest that the fit is substantial. Nevertheless the reader is permitted no over-all assessment of the fit, and short shrift is given these external stimulus conditions in the conceptual development.

The author may intend to restrict close attention to the level of psychological process. If this be the case, then the proof of the pudding remains for the second thesis, i.e., that the current reality world affects the perception of events. The Hungarian revolt forms an excellent test case. While group mediation of information must have been important in determining the impact of this event, we would suppose that a thorough understanding of individual reality worlds would help us predict differential reactions to it.

INSTEAD of prediction, however, the fact of differential reaction is accepted as a given, after a gratuitous re-assertion that such reactions did indeed depend upon assumptions brought to the event. The twofold classification of voters now splits to five, in order to accommodate the variety of reactions within both militant and protest camps, and Cantril marshals further case studies, which informally describe the cognitive restructuring imposed by the event within each category. Brief mention of differences in education, economic security, and group dependence are left, as before, to carry the bulk of the explanatory weight.

As a psychologically sophisticated reporting of the grievances which have underpinned Communist voting in Western Europe, the book recommends itself to the student of political behavior. Its description is rich and provocative, and its conceptual tools put but little strain on raw reality. They are not, however, applied in a manner calculated to insure the reader that they are the best tools to advance his generic understanding of behavior in this setting.

Ecce Homo

Hannah Arendt

The Human Condition. Chicago: University of Chicago Press, 1958. Pp. vi + 333. \$4.75.

Kenneth Galbraith

The Affluent Society. Boston: Houghton Mifflin, 1958. Pp. xii + 368.

Reviewed by BERNARD MAUSNER

Dr. Mausner is Research Associate in the Graduate School of Public Health at the University of Pittsburgh. For a long time he has been interested in social interaction and just now he is principal investigator in a new project in that field. CP's readers will remember what he had to say about the social reaction to fluoridation (CP, Dec. 1957, 2, 313f.). With three others he has published a book on fact and opinion about people's attitudes toward their jobs (CP, July 1958, 3, 192f.). Presently John Wiley will be publishing another book in which Mausner is a co-author, a book to be called The Motivation to Work.

ONE of the central problems of psychology is the search for an understanding of motivation. Unfortunately most academic psychologists focus on the form rather than the content of motives; that is to say, they are so concerned with the development of a conceptual framework that they have little to say about the *what* of motivation as contrasted with the *how*. The two books with which this review is concerned are excellent correctives to this tendency. They are contributions by eminent nonpsychologists to the study, not of 'motivation' but of human motives. As such, they merit careful attention by psychologists.

Dr. Arendt, the author of the first, defies conventional pigeon-holing. She is a roving philosopher, political scientist, economist, and historian. She displays a remarkable virtuosity in the citation of her enormous acquaintance with the lit-

erature of the Western world; but her book is even more remarkable for the originality and cogency of its thoughts than for the way in which they are supported. She writes well, albeit in a rambling and discursive style. The reader who knows little Latin and less Greek may gag on occasion at the rich brew distilled by her scholarly apparatus, but he will be the better for it if he swallows the draught and moves on. This is not an easy book to read; nevertheless, it ought to be finished rather than merely skimmed.

The book begins with a statement of the basic structure of human life. The *vita activa* is that part of life in which man is, indeed, active. It is contrasted with the life of thought, the *vita contemplativa*. The bulk of the book is concerned with the close specification of the three components of the *vita activa*. The first of these, labor, is activity which leads to the creation of goods whose sole end is to be consumed. Labor is closely tied to the biological aspects of life, to the rhythms of hunger and satiation. Labor is associated with pain, as in the toil of the husbandman or the travail of the woman in childbirth. It is contrasted, in all languages, with something called work (*arbeiten* as against *werken*, *travailler* as against *ouvrer*). Work produces objects of lasting worth, as against the transient products of labor. The worker, in his pride of craftsmanship, creates works (*oeuvres*, *opera*) which, in their enduring life, are more than mere objects for consumption. The end of work is achievement; the end of labor is "that effort and

gratification follow each other as closely as producing and consuming the means of subsistence, so that happiness is a concomitant of the process itself, just as pleasure is a concomitant of the functioning of a healthy body." Yet only work can produce objects which have enduring form or shape, beauty or ugliness. Work is the product of an image in the mind of the worker; as with the Platonic myth of the cave, the idea of the object becomes transformed over and over again into the imperfect reality of the fabricated form.

YET work and labor alone are inadequate to describe the entire realm of human activity. The poet or the flute player, the teacher or the general do not produce consumables nor, except in a metaphorical sense, do they carry out acts of fabrication. Their acts are, nevertheless, at the highest level of human potentiality because, played as they are on a public stage, they affect persons other than the actor and leave behind a residue of recollections. It is in action that the individual is most unique; his goal is reputation in the present, and a place in history in the future. Here man is most human. "The public realm, the space within the world which men need in order to appear at all, is therefore more specifically the work of man than is the work of his hands or the labor of his body." To the laborer life and the consumption which supports it is the highest goal; to the worker the product of craftsmanship is greater than the craftsman himself. To the man of action the greatest end is encompassed by humanity itself, the self-actualization of the actor.

The highest point of human civilization was reached in the Greek city-state where the citizen, freed of the necessity for labor by the institution of slavery, acted on the stage provided by the *polis*. It was only in the genuine democracy of the *polis* that action could flourish. After the Periclean epoch, even political theory failed to provide an adequate understanding of this human need. The Platonic notion of an extension of the family's social structure, with its rulers and ruled, into the state was the antithesis of the *polis*. With

the destruction of the *res publica* in the later days of Rome and the denigration of the public realm, the glorification of the immortality of the soul rather than of the reputation, which characterized Christianity, the decline of action was hastened further.

The relative importance of work was enhanced by the Industrial Revolution which, in the profusion of its advances in technology, expanded enormously man's capacity to shape the raw materials of nature into objects of enduring worth. Thus, with the triumph of the worker, *homo faber*, the maker of tools and vehicles, became a more significant member of society than the statesman or the epic poet. Ironically, this very triumph included the seeds of its own destruction. With the full growth of industrial civilization, labor triumphed over work. The destruction of craftsmanship and the growing rationalization of industrial activity meant that workers in the old sense became increasingly scarce. Sheer consumption has become the overriding end of society; only survival itself has any significance. Since one of the greatest contributions of industrialization has been to remove the pain and toil from labor, in the Western world at least, labor has grown increasingly remote from its primal sources of strength in the biological cycle. Our society has turned into an aggregate of jobholders.

In the ancient world the *vita contemplativa* supplied the rationale for the action, and the life of action supplied a goal for the activity of work. As a final blow we have the destruction of the *vita contemplativa*. When Galileo searched the skies, the race learned that reality could no longer be tested by human senses alone. Scientific and philosophical truth parted company for all time. Such 'reality' as could be found came only from the manipulations of the scientist.

GALBRAITH has written an entirely different kind of book, one limited to the here and now, facile and determinedly witty. Yet the ideas are essentially parallel to Arendt's. Galbraith attacks the 'conventional wisdom'—those truths in long gray beards which have

outlasted the time in which they were developed. The most pernicious of these is the notion, based on the long history of human privation, that production for its own sake is the highest goal for our own society. To maintain production at high levels in our present American economy calls for an intense stimulation of a demand for consumables. The presumed end is full employment and national security. The sheer concentration on production without critical attention to *what* is produced, and the value system which denies the value of public production and exalts the private sector, leads to a social imbalance in which schools and hospitals are inevitably starved in favor of finny automobiles and TV sets. The national security is imperiled by our inability to produce the means for our defense.

In contrast to Arendt's broad sweep, we focus down on the immediate problems of contemporary America as Galbraith examines the operation of a society which revolves around labor and its end, the cycle of consumption and production. Although he excuses himself from the need to suggest therapy, he goes on to sketch some solutions. In essence, these consist of a search for devices with which to make practicable the reawakening of public goals. This reviewer is not competent to judge the excellence of these devices. Galbraith also hopes that a 'new class' of people will arise for whom labor is not necessary. These, whom he describes in much the same terms used by Arendt for "workers," will find renewed meaning in the act of work itself and so can be withdrawn from the meaningless round of producing for consumption's sake objects for which a demand must be created artificially.

Arendt's diagnosis is masterful; her statement of the dilemma of the modern world is convincing. Yet she has no real denouement. Her book ends with a page and a half of facile optimism in which the craftsmanship of the artist, and the action of the scientist on a new stage are named as sources of renewed hope for humanity. Galbraith's solutions, too, carry less conviction than his polemic; it is axiomatic that exhortation is the poorest form of therapy. There is little in *The Affluent Society* to

give us hope that the deep-seated attitudes it describes are easily susceptible to change.

Both authors imply the need for a new revolution. Just as the industrial revolution freed labor from its accompaniment of painful toil, a new stage of mankind would find labor returned to some of its early meaningfulness drawn from the rhythms of biological need. It would find craftsmanship reawakened in a new sense of the dignity of work and, in the building of a new *polis*, create new goals for mankind. But how? As Arendt has so clearly shown, the Galilean probe into the skies has closed for all time the *vita contemplativa* as a road to these goals. In a world of Xi-zero particles and behaviorist psychologies, the inner visions of poets and philosophers will never again provide meaning for humanity. Are there any substitutes?

From Core to Cortex

H. W. Magoun

The Waking Brain. Springfield, Ill.: Charles C Thomas, 1958. Pp. viii + 138. \$4.75.

Reviewed by ROBERT J. ELLINGSON

who is Associate Professor of Medical Psychology in the College of Medicine of the University of Nebraska, and Chief of the Electroencephalographic Laboratory of the Nebraska Psychiatric Institute. He was introduced to the brain by Donald Lindsley, whose assistant he was at one time, and Lindsley at Northwestern University was a collaborator of Magoun's. Ellingson reviewed Cleghorn's volume about the use of scientific techniques in psychiatric research, reviewed it in CP last year (June 1959, 3, 156f.).

PROFESSOR MAGOUN has spent the greater part of his scientific life exploring the highways and byways of the brain stem. The work that he has done

with a series of distinguished collaborators—ranging from the late Stephen W. Ranson to the group of able colleagues and students gathered round him today at the University of California at Los Angeles—constitutes an outstanding contribution to the fields of neuroanatomy and neurophysiology and, as the book under review shows, to psychology as well. His career further provides a notable example of interdisciplinary, collaborative research at its most productive level, a condition attained only when collaboration arises out of a spontaneous and sincere mutual interest in the problems at hand.

In the mid-1940s, while he was Professor of Anatomy at the Northwestern University School of Medicine, Magoun concerned himself with the neural mechanisms involved in the production of spasticity, and in 1947 he published (with Ruth Rhines) a monograph outlining with superb clarity the major factors involved in the production of that disorder. There followed in 1949 a paper (with Donald B. Lindsley, a psychologist, and Leon Schreiner, now a neurosurgeon) more precisely delineating the role of the lower brain stem reticular formation in the inhibition and facilitation of the activity of lower motor neurons. The Northwestern group, in collaboration with Professor Moruzzi of Pisa, then turned its attention from the 'downstream' to the 'upstream' influences of the reticular formation, and in late 1949 and 1950 there appeared a series of three now-classic papers establishing incontrovertibly the importance of the reticular formation in the production and maintenance of wakefulness. It is safe to say, from the vantage point of almost ten years, that these experiments virtually opened a new era in the neurophysiological and neuropsychological study of the brain.

In 1950 Magoun accepted the professorship of anatomy at the new Medical School of the University of California at Los Angeles, where he has been the center and guiding spirit of one of the most versatile and productive teams of scientists working on the problems of brain function anywhere in the world. In 1956 he was invited to give the Salmon Memorial Lectures at the New York Academy of Medicine. *The Wak-*



H. W. MAGOUN

The apparatus is a precise replica of the original stereotaxic instrument devised in 1908 by Sir Victor Horsely and R. H. Clarke for the exact localization of a point within the brain's substance. The replica, made by the Palmer Instrument Company of London in 1909 for Dr. Ernest Sachs, was presented by him to Dr. Magoun at Yale University in a ceremony in 1956.

ing Brain comprises the text of those lectures, substantially as presented. An amazing amount of experimental data is summarized in a few pages of clear and extraordinarily concise prose. In fact, the book's only defect is that the material is in parts so concentrated (undoubtedly due to the limitations of the lecture form and the nature of the original audience) that it may be difficult for those not at home in the nervous system to follow. Beginning students will need help to understand. Reading the *Conclusion* first is a good idea.

Of the many topics that will be of interest to psychologists there is space to mention only three. Neural mechanisms related to attention come in for consideration at several points. It has been shown that central control of incoming impulses is exerted much farther downstream than many of us would have suspected a few years ago: at the first synaptic relay or even in some instances in the sensory end-organ itself. Further, the capacity of the more cephalic, thalamic component of the reticular system for fractionated, shifting influence upon focal regions of the fore-brain provides a possible mechanism for attentional processes at the highest neural levels.

There has been some clarification, as well as complementation, of the classic

data on neural mechanisms of emotional behavior. The cephalic brain stem and paleocortex appear to be heavily involved. Magoun reviews the more recent researches and sums up: "Without further information, it would appear naive to propose contrasting EEG patterns for emotion and for intellect, or to suggest the rigid parcellation of these faculties in older and newer parts of the forebrain, though considerable support seems available for this latter concept."

Finally, studies dealing with electrical brain changes during conditioning, and the inferences which can be drawn con-

cerning the reticulo-cortical mechanisms involved are discussed. Some of the considerable literature on this subject which has appeared since these lectures were given is cited in the very useful bibliography of almost 200 items.

This little volume comes as close as any can to being 'must' reading for those who are interested in brain function from any aspect and who are not already conversant with the original literature upon which it is based. Even sophisticates may find information they had overlooked (for the original literature is widely scattered) and profit from the author's insights and interpretations.

ries. (Does Bronfenbrenner believe that mutual discovery and love between persons can only be a value and cannot be a fact?)

It is unfortunate that Mrs. Lynd, who discusses seriously and thoroughly questions of fundamental importance to psychology today, should be labeled in the only journal of the American Psychological Association which reviews books as employing methods which are "less those of science or even social philosophy, than of literary criticism," that the review should be headlined *In Dispraise of Fact*, and that Mrs. Lynd should be accused of wanting us to "equate complexity with confusion, precision with superficiality, and the tangible with the trivial." I realize that there are some psychologists who can look at reality from only one point of view. To look at a subject from any additional sides is for them unthinkable, unscientific, or both. Such traditionalists may well find Mrs. Lynd's broad and multifaceted approach threatening. Psychologists who are not so fettered will, I believe, be stimulated and even excited by Mrs. Lynd's book and may well judge it as I do: a real contribution to our growing understanding of personality.

NANCY MORSE SAMELSON
Ann Arbor, Michigan

ON THE OTHER HAND



SHAME AND IDENTITY

Urie Bronfenbrenner's review of Helen Merrell Lynd's *On Shame and the Search for Identity* (CP, Apr. 1959, 4, 114f.) seems to me to be misleading and unfair. In her book Mrs. Lynd explores two important yet relatively neglected aspects of personality: shame and identity. She has not attempted to write a definitive book on this dual subject and describes the book herself as "cumulative speculations on a subject," taking the form of questions and suggestions rather than final conclusions. She wisely realizes that the most scientifically useful first step in such uncharted territory (and it is my belief that psychology still includes a great deal of such territory) is to begin to map out the major characteristics of the phenomena and to make a preliminary examination of some of the surrounding area. Her search for further understanding leads her to approach the phenomena from a very wide variety of ways, including the use of literary sources, an atypical approach for psychologists.

One of the directions her thinking takes is the examination of the question of why shame and identity have been largely overlooked in psychology. After considering a number of contemporary psychological

theories and their underlying assumptions, she suggests that the relative neglect of both shame and identity may be related to the difficulty of accommodating such phenomena within theories containing certain types of assumptions about human behavior—assumptions which are widespread in psychology today. (This critique of assumptions underlying much of present-day psychology is, I feel, one of the best sections of the book and would be, for example, a valuable supplementary reading in courses dealing with personality theory.) One of Bronfenbrenner's major lines of attack in his review is described by his statement: "Mrs. Lynd has little interest in the 'world of facts' and yet the basis of Mrs. Lynd's critique of current assumptions about human behavior is that they run counter to empirical evidence. Furthermore she turns to certain conceptions of personality because they are more fully in accord with the evidence, i.e., the facts about human behavior."

It seems to me that Bronfenbrenner also fails to understand Mrs. Lynd's inclusion of such phenomena as "the possibility of mutual discovery and love between persons." Again she is directing herself to vital human experiences which are usually ignored or dealt with only very peripherally by most contemporary psychological theo-

The review of Helen Lynd's book in the April issue of *Contemporary Psychology* by Urie Bronfenbrenner (CP, April 1959, 4, 114f.) distressed me. The title, for instance, for which he himself was probably not responsible, does injustice to a book which I believe is concerned with the fact of human existence.

Mrs. Lynd is criticized for having "little interest in the 'world of facts.'" Perhaps this is true, if a fact is defined in the Pearsonian sense; but Helen Lynd operates within the field definition, one which is accepted in many areas of science, including psychology. For her, the fact is not pure datum; it is an artifact created by man out of the data of experience. For such a definition of fact, full context is necessary; and this she finds in literature more often than in so-called scientific studies where the context is contrived and limited.

If she approaches the 'facts' of Freud and Aristotle in a spirit of criticism and inquiry, it is because she regards the observer, the interpreter, the feeling individual, the operations through which a conclusion is reached, all as a part of the fact; and tries to take account of these so that she can arrive at the ultimate datum.

On page 115, Mrs. Lynd is contrasted

with Köhler who advocates "a scientific phenomenology." Does this imply that her approach is not phenomenological? or merely not scientific? If the former, I would like to say that, to my mind, actually the very aspects which Dr. Bronfenbrenner deplors are those which show that—to use Comb's phrase—her interest "lies not in the event but in the phenomenon, that is to say, in the individual's experience of the event." And I believe that this, the immediacy of sensuous experience, is even Hegel's understanding of phenomenology.

Dr. Bronfenbrenner refers to Mrs. Lynd's work as superficial. Does he mean that her approach is superficial? I have lived through this work with her for the last few years, and I have found her interest to be profound and her search incisive. In my correspondence, I find her first letter referring to this search dated July 1951, in which she speaks of "a problem I have been thinking about for the last five or six years." When I saw her manuscript soon after this, it was a thin sheaf, and it had grown only a little thicker in 1954

when I saw it again, though she had been hard at work at it during the intervening years.

Nor can I agree that Helen Lynd makes no reference to non-Western thought. Actually, she makes many references to my own work with Trobriand and Wintu material, and the Wintu Indians are only geographically in the Western world. Even the Greek concept of shame which she studied at length and intensively is, I think, closer to the Middle Eastern sense of shame and identity than to the Western world (and for a Greek to make such a statement is tantamount to treason). My own impression of the Bedouin as presented by Doughty, and of children of Syrian parentage studied by the Columbia University Research Project in Contemporary Cultures, is that though the sense of shame is expressed differently in each of these groups, it shows close similarities at base among them all. The State Department, which includes Greece in the Near East, considers the historical and cultural factors that associate Greeks with the Near East outstanding. Technically, of course,

Greece is in Europe; but that does not mean that the Greek sense of *entropé* and *philotimo* are to be classified as Western.

DOROTHY LEE
Harvard University

AFTEREFFECTS: FIGURAL AND NEGATIVE

In his review of my monograph on figural aftereffects (*CP*, Sept. 1959, 4, 294f.), Professor Gibson suggests that, by crediting him with their discovery, I have helped to perpetuate a confusion between *figural* aftereffects and *negative* aftereffects, which are dimensional and following adaptation or normalization. I have to admit that the monograph is inconsistent in crediting him, on the first page, with the discovery and in expressing doubts, on almost the last page, that Gibson's effects really are figural aftereffects. In experiments on figural aftereffects, I have always been careful to avoid visual patterns which might be affected by normalization. To that extent I have continued to have reservations on this matter.

WILEY

BOOKS



The Language of Psychology

By GEORGE MANDLER, *Harvard University*; and WILLIAM KESSEN, *Yale University*. Offers an analysis of scientific language in psychology, tracing its development from the everyday vernacular to the formulation of explicit theories. It deals with such contemporary issues in philosophy and psychology as phenomenology, operationism, definition, induction, the nature of explanation, and the creation and structure of scientific theories. The authors emphasize recent developments in the logic and philosophy of science, and discuss the use of psychological findings and theory in an understanding of traditional philosophical problems. 1959. 301 pages. Illus. \$6.75.

The Social Psychology of Groups

By JOHN W. THIBAUT, *University of Carolina*; and HAROLD H. KELLEY, *University of Minnesota*. This book is designed to bring order and coherence to present-day research in interpersonal relations and group functioning. To achieve this aim the authors employ simple concepts and principles which are defined and illustrated explicitly, and introduce new or more complicated concepts only when it becomes necessary. Thus, the book begins with an examination of one of the simplest forms of social phenomena, the two-person relationship or dyad. The analysis of the formation of a dyadic relationship and the consequences of the pairs' continued interaction is used throughout the book to illuminate larger problems. 1959. 313 pages. Prob. \$7.00.

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Since reading Gibson's review, I am satisfied that discussions of figural aftereffects, following the account by W. Köhler and H. Wallach (*Proc. Amer. Philos. Soc.*, 1944, **88**, 268-275), have underrated the problem raised by Gibson's work and that the interest aroused by figural aftereffects has contributed to the neglect of a most valuable method of research. Yet I do not think that Gibson is entirely free from responsibility for confusion. So far as I know, his review of my monograph is the first attempt he has made to have the situation remedied and, in making it, he has not commented on the evidence which is mainly responsible for the confusion between the two aftereffects.

Gibson measured the aftereffect (on a vertical test-line) of the inspection of a line tilted at 10° to the vertical. Köhler and Wallach repeated this and also reversed the positions of inspection and test-lines. In this latter case, since the inspection line coincides with the vertical norm, no normalization, and consequently no negative aftereffect, is to be expected. Yet the aftereffects in all three cases were of approximately the same magnitude. Again, Gibson found no negative aftereffect with a line tilted at 45° , yet Köhler and Wallach found a rather large aftereffect, presumably figural, with this line and a test-line tilted at 35° to the vertical. There are, therefore, aftereffects with tilted lines under conditions appropriate for both negative and figural aftereffects but there are other tilted line-effects which can only be figural ones. In these circumstances there is some justification for Köhler and Wallach's considering that the tilted line effects are figural aftereffects. The main argument against this view is the occurrence of tilted-line normalization. Since negative aftereffects are demonstrable with other normalizations, they must be expected in this case also. This argument implies that, if normalization of tilted lines occurs under Gibson's conditions, then some aftereffects must be both negative and figural.

Gibson and M. Radner (*J. exp. Psychol.*, 1937, **20**, 453-467) appear to have observed normalization as a preliminary to their main experiments but in these they measured only aftereffects and the condition for doing this permits the development of figural aftereffects. M. D. Vernon (*Brit. J. Psychol.*, 1934, **25**, 186-196) also reports normalization but she used conditions differing slightly from Gibson's. W. C. H. Prentice and D. C. Beardslee (*J. exp. Psychol.*, 1950, **40**, 355-371) measured normalization by having subjects compare the tilted inspection-line, at in-

tervals during inspection, with an equally tilted line briefly exposed on the other side of the fixation point. Their results confirmed Gibson's but their arrangements have been criticized as not excluding the possibility of 'satiation.'

There are similarities in the procedure necessary and some other pieces of information which suggest that the tilted-line aftereffect and the figural aftereffect are not very different from one another. For example, Gibson measured the growth of the tilted line aftereffects as a function of inspection time. E. R. Hammer (*Amer. J. Psychol.*, 1949, **62**, 337-354) did likewise for figural aftereffects using a 'figural' pattern. Both found measurable aftereffects within five seconds and maximal effects at about one minute. These times are rather different from those found necessary for the adaptation processes investigated by Erissman and Ivo Köhler.

It seems that there are two areas of research, each now involving a considerable body of information, with some features of procedure in common, which come together in Gibson's curved, bent, and tilted-line effects. While there is no clash elsewhere between the notions concerning adaptation and normalization, on one hand, and figural aftereffects and 'satiation,' on the other, in this connection both sets of notions lead us to expect the same, and the obtained, results. Although it is quite conceivable that there is no interaction between the processes concerned, yet it seems worthwhile to inquire into this matter. Other hypotheses than peaceful coexistence are possible, and investigation may turn up something interesting. The word *figural* ought not to impede our thinking. Its use can be appropriate only for visual aftereffects and there seems no good reason, as yet, for not regarding effects in other modalities as also *figural*.

PETER McEWEN

The Queen's University, Belfast

DEAR DRs. GUTTMAN AND SCRIVEN:

I have followed your skirmishes in *CP* with the joy of a lover of a good fight. But after a while, as any fight fan will tell you, what the crowd wants is for the pugilists to quit sparring and 'mix it up.' Since, as I see it, it is only possible to spar in *CP*'s ring, let me suggest that you exchange these lefts and rights (and research) as long as necessary and then publish them in a monograph or book. I'm looking forward to a main event. In fact, I can even speculate on *CP*'s review of the battle.

JOHN G. MILLER

University of Missouri

A RED-FACED GOAT

This is *my* title. I use it because it is currently self-descriptive. In my recent comments (*CP*, July 1959, **4**, 222f.) on Schmeidler and McConnell's *ESP and Personality Patterns* and Scriven's review thereof (*CP*, Oct. 1958, **3**, 295f.) I erroneously interpreted "between-persons" variance as ordinary "within-groups" variance. This led to the erroneous criticism that two *ts* were based on inflated *Ns*.

For this blunder and the remarks based thereon, I extend my apologies to Drs. Schmeidler, McConnell, and Scriven and to *CP*'s readers.

The correctness of the *ts*, however, accentuates my original major question: How can ESP, measured with near zero-reliability, be correlated with the sheep-goat dichotomy to the extent of .15 and .37 (biserials), for group and individually collected data, respectively? Even if that second *r* were computed as a point biserial, .28, the attitude variable (believers-skeptics) 'explains' a percentage of obtained ESP score variance that is larger than the percentage attributable to *true* ESP score variance.

This absurdity is not independent of the fact that for the sheep alone, $N = 111$, the reliability is $-.20$; that is to say, the between-persons variance, which supposedly contains error variance as a component, is smaller than the latter. One way that this could occur would be for some of the sheep to have been so designated on the basis of their ESP performance, a possibility which emerges as a likelihood when we read Schmeidler and McConnell's (p. 125) admission that such contamination may not have been controlled. Who knows? Maybe this contamination explains the highly significant *ts*.

QUINN MCNEMAR
Stanford University

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